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CERTAIN ASPECTS OF HIGH-PRESSURE RESEARCH.*

BY
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As some of you know, I have been occupied for a number of years in determining the effects of high hydrostatic pressure on a number of physical properties. To-day, I propose to stop for a minute for a general stock-taking, to outline briefly some of the results obtained, and to suggest partially their possible significance. I am impelled to do this because the variety of subjects touched by this high-pressure research is so great, and the character of the information obtained in many cases so special, that the whole effort is likely to appear without general scheme or significance. But I believe that different limited aspects of the subject will be of interest to different individuals; by presenting to you the general outline, I hope to make it possible for any individual to become aware of those aspects of the work which may be of special significance to him.

My general attitude in my investigation has been that high pressures are a tool which may be significantly applied to the discussion of many different problems. It is not usual, however, for an experimenter to devote himself to exploiting a single tool of research. I am somewhat in the position of a small boy with a new jack-knife who rushes about trying it on every conceivable object. Doubtless the more mature method is to maintain a tool-box from which one might take the jack-knife when one has

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to sharpen a pencil, or an auger when one has to bore a hole. I may suggest, in partial extenuation of my attitude, that there seem to be very few jack-knives in the world, or at least very few people who seem willing to use them, and there really does seem to be a certain amount of whittling that needs to be done.

The kind of problem which can be attacked most successfully by the use of high pressures is not that which is now most interesting to most physicists, nor which appears to be most fundamental. This tool is not particularly adapted to probing questions of the constitution of the atom and particularly of the nucleus, but is better adapted to the larger scale atomic and molecular phenomena. There are, of course, occasions when the behavior of matter under high pressures intimately involves the inner structure of the atom, but in general the pressures with which I am concerned are too small to produce large changes in the atoms themselves. We are concerned here with pressures from 10,000 to 20,000 atmospheres. These are capable of producing comparatively large and significant changes in the properties of many substances, but they cannot, of course, be compared with the pressures of perhaps billions of atmospheres which occur in the interior of the stars and which are supposed to be a factor in atomic evolution.

The significance of high pressure as a tool of atomic research lies in the extreme simplicity of the change it produces in the external conditions, merely pushing the atoms closer together so that they are compelled to give a somewhat more intimate account of their own inmost selves. The changes produced by pressure are obviously much simpler than those produced by a change of temperature. No one regards our understanding of a phenomenon as satisfactory unless we are able to give some account of the changes produced by temperature; still less should we be satisfied if we cannot account for the changes produced by pressure. Further, by combining the effects of temperature and pressure, we extend enormously the field in which we may look for phenomena suggestive for our theories, for the field is now two instead of one dimensional.

It is not necessary to apologize for activity in atomic phenomena, even if they are not on the extreme end of the exploring line, because they are the phenomena which are perhaps of most immediate concern to us in daily life, and we have as yet hardly

begun to understand them. The extent of our ignorance may be emphasized by the fact that none of our theories is able to predict accurately any of the high-pressure phenomena which I shall summarize, in spite of their extreme simplicity.

Perhaps the most fundamental of the changes produced by pressure are those of volume itself. I have measurements of the compressibility of five gases, fifteen liquids—most of them simple organic liquids but including water and mercury—and some forty-five different solids, thirty-five of them metallic elements, several minerals and several varieties of glass. I may mention here also the compressibility of eleven of the alkali halides determined by Dr. J. C. Slater, in the Jefferson Laboratory. All of these compressibilities have been determined at several temperatures, so that we know the effects of pressure on thermal expansion. The melting curves of thirty-seven substances, including metallic elements and organic compounds, have been determined. These melting measurements include a determination of the change of melting temperature when pressure is raised, the change of volume on melting, the latent heat of melting, and in many cases the difference of compressibility between liquid and solid. The effect of pressure on the polymorphic transitions of some thirty substances has been investigated, including organic and inorganic compounds and the very interesting case of water. This investigation has involved the study of sixty-nine transition lines and twenty-one triple points. On these transition lines, the change of transition temperature with pressure has been determined, and also the change of volume, the latent heat, the difference of compressibility (which gives information about the difference of thermal expansion and specific heats), and the effect of pressure on velocity. In addition to these thirty substances, about one hundred others have been examined, and no new polymorphic forms have been found. The effect of pressure on electrical resistance over a range of temperature has been determined for forty-five metallic elements in the solid state, eight in the liquid state, and for seven alloys. The effect of pressure on thermal e.m.f. has been measured for eighteen metallic elements and two alloys. The effect of pressure on the thermal conductivity of eleven solid metallic elements, six minerals and fifteen liquids (all organic except water) has been measured. Finally, a few obser-

vations have been made on the phenomena of rupture under high pressure.

What now is the general nature of the facts found, and some of their implications? First, consider the facts with regard to changes of volume. At high pressures there is no essential difference between a gas and a liquid. Under 12,000 kg. the changes of volume of ordinary non-metallic liquids range from something like 20 per cent. for water and 33 per cent. for ether, up to those of hydrogen and helium, which are the most compressible substances investigated, and which, under 12,000 kg. at ordinary temperatures, have a volume less than half that which they have when frozen solid at atmospheric pressure by temperatures close to 0° Abs. Under these pressures there is a very great change in properties. The thermal expansion drops to less than one-fourth of what it is ordinarily, and the compressibility drops to one-fifteenth; both of these changes are very much greater than the changes of volume. The fact that compressibility and thermal expansion drop off so greatly for comparatively small changes of volume suggests that these must be intimately connected in some way with the empty spaces between the atoms. All of the organic liquids behave roughly alike, so that an average of them furnishes us with a sort of ideal liquid, analogous to the familiar "perfect" gas. This ideal liquid has one curious property; above a certain pressure the ordinary behavior of thermal expansion reverses itself, and the liquid is more expansible at the low rather than at the high temperatures. In addition to the broad features, which are alike for all liquids, each individual shows small and perfectly characteristic divergencies from the mean, which do not become conspicuous until the highest pressures are reached. These suggest differences between the different kinds of atoms or molecules, roughly corresponding to differences of shape. If an atom has a "knob" on it (local inequality in its force field), it will interfere with its neighbors and produce perceptible differences of behavior when the atoms are pushed close together by high pressures. On the other hand, it is interesting that water, which under ordinary conditions is a highly abnormal liquid, under high pressures loses its abnormalities and returns to normality.

The kinetic theory of gases, extended to include the behavior of liquids up to a few hundred atmospheres around the critical

point, has engaged almost exclusive attention up to the present time. There is, so far as I know, no theory of liquids that, with any approach to success, attempts to picture how the molecules in a liquid behave when pushed into such close contact that there is considerable mutual interference.

The changes of volume of the solid elements (metals mostly) are in general much less than those of liquids, but they are nevertheless significantly large, as in most cases it is possible to reduce the volume by pressure to considerably less than it would be if the metal were deprived of all heat motion by being cooled to absolute zero at atmospheric pressure. Some of the metals are highly compressible, and cæsium, in particular, loses more in volume at high pressures than ether. There is a characteristic difference between liquids and solids in that solids do not lose their compressibility nearly as rapidly at high pressures. We would expect a difference of this sort; in solids, the atoms must retain their regular crystalline arrangement at high pressures, so that the free spaces around the edges and corners of the molecules are never occupied and hence play a relatively small part in the phenomena of compression. In a number of solids it seems that the persistence of compressibility at high pressure can be explained only by a compressibility of the atoms themselves. There is here an important problem for quantum theory: To formulate the quantum conditions when the electronic orbits interfere with each other as much as they must in a strongly compressed solid.

There is no adequate theory to explain the properties of metals. In fact, the general nature of their structure is less well known than is that of the structure of certain salts, compounds of two elements. The data on compressibility now give us the means of finding the first three derivatives of the forces between the atoms of a metal. As a physicist, I am sanguine that the mathematician can go far toward reconstructing the function if he knows its first three derivatives, and we may hope at any rate that these data will make possible a more adequate description of the force field about an atom, even if they do not completely determine the mechanism.

It has been stated that compressibility becomes less at high pressures. This is what we should expect, because, as the atoms are squeezed closer together, it seems natural that they should

resist more strongly attempts at closer propinquity. I have found, however, a significant exception in quartz glass and several varieties of ordinary glass in which SiO_2 is a prominent constituent. These become more compressible at high pressures. This cannot but be significant; our theories must explain why it is that, under some conditions, the atoms resist compression less strongly the closer they are pushed together. It is natural to seek to establish a connection with the fact that these abnormal substances are amorphous instead of crystalline, and that they would doubtless crystallize with a decrease of volume.

The probable effect of pressure on melting temperature was for a long time a matter of controversy. Early views were strongly influenced by the striking critical phenomena between liquid and vapor, and it was supposed by many that there would be found a critical point between liquid and solid such that above this point liquid and solid might be made to change from one into the other without discontinuity. A later rival theory, strongly defended, was that of Tammann, who supposed that there was a maximum melting temperature above which a liquid could not be forced to freeze by any pressure no matter how high, and below which the liquid might be frozen and the solid melted again by a sufficiently high pressure. The experimental difficulty in settling this question was that the available pressure range was not sufficient. But with the data now at hand, we may, I believe, regard this question as settled. It appears from the evidence of thirty-seven substances that there is no experimental reason to think that the melting curve does not rise indefinitely, at a continuously decreasing rate, but nevertheless so that at any temperature a pressure can be found high enough to freeze the liquid to the solid. There is no connection whatever between the melting phenomena and the ordinary critical phenomena of liquid and vapor; the amorphous phase may be frozen by sufficient pressure either above or below the critical temperature. This is, after all, the natural result. It would seem as if one ought, by pushing the atoms close enough together, to be able eventually to undo the disorienting effect of an increase in the energy of temperature agitation. A continuous passage from a liquid to a solid was most difficult to visualize with our concept of a liquid as a haphazard aggregate of molecules, and of the solid as consisting of the same molecules regularly arranged.

The discovery of the true character of the melting curve is of evident importance for geophysics.

In addition to an indefinitely rising melting point, the measurements showed that solid and liquid become more and more alike in volume at high pressures, but that the latent heat absorbed on passing from solid to liquid is little changed. It is furthermore universally true that the liquid is more compressible than the solid. This is natural enough for normal liquids, but is not so obvious in the case of ice, which has a volume larger than that of liquid water, but is less compressible. All these facts must be explained by the future theories, but nothing essentially new is involved here; when we have an adequate theory of the solid and the liquid state separately, the correct explanation of the melting phenomena will automatically follow.

Another interesting fact brought out by the measurements on water and some other substances which crystallize in two or more modifications is that in the liquid there may be nuclear structures of considerable complexity capable of persisting for days. The liquid is not the simple thing that it may appear to a casual glance, but, at least under some conditions, it may carry concealed within it traces of its past history imperceptible to ordinary means. The existence of these structures in the liquid is now being demonstrated by X-rays, but the extraordinary persistence of the individual structure cannot be shown by such means.

One minor way in which the pressure tool may be of value is in determining latent heats of melting. A calorimetric measurement is admittedly difficult. By measuring the melting pressures corresponding to two melting temperatures near the normal melting point and determining the changes of volume, one has an indirect measurement of latent heat which is often far preferable to a direct measurement. The pressures involved are not high—only a few hundred atmospheres—and the apparatus is so simple that it may be readily constructed.

Contrasted with the phenomena of melting are those of polymorphic transitions in the solid state. These are of all degrees of complexity and are governed by few simple rules. No cases have been found of a critical point between two solid modifications; this doubtless corresponds to some essential physical necessity, for it is hard to see how one kind of space lattice which is characteristic of one crystalline modification can change, without discontinuity, into some other space lattice

characteristic of another crystalline modification. Except for this, we may apparently have nearly every kind of behavior. Under varying conditions of pressure and temperature, transitions may take place with no volume change, and others with no latent heat. As pressure rises, the slope of the transition line may either rise or fall, while the slope of the melting curve always falls. There is a surprisingly large number of cases analogous to that of water and ice, in which the modification stable at the higher temperature has the smaller volume. We might be inclined to think of this as an abnormal state of affairs, and might expect that high pressure would tend to wipe it out, but such is not the case, and these ice-type transitions persist at least to pressures beyond our present reach. Another surprising fact is that in the majority of cases the modification with the smaller volume is the more compressible. This is the same behavior as that shown by water and ice, and cannot help being of significance for atomic structure. It means that the atom has outlying parts which have centres of force of strength sufficient, in conjunction with the outlying parts of other atoms, to build up an open-work structure, and that these outlying parts have more than usual stiffness, so that the resultant open-work structure has a lower compressibility than the more dense structure which the atoms are forced to assume by a pressure sufficiently high to push the outlying centres of force out of register. From some points of view, the thought of atoms as having "knobs" is certainly suggestive, although so crude a picture is repulsive to some persons.

One feature of the behavior of polymorphic forms, which is of great significance, may be studied to advantage with the high-pressure tool. This is the speed with which one modification transforms itself to another. A particular advantage of this tool is the nicety and rapidity of control which it offers. Changing the pressure on a two-phase system is equivalent to changing the temperature, but, whereas the actual temperature can be changed only slowly and non-uniformly throughout the interior of the apparatus, and considerable time is required for attainment of equilibrium, the effective temperature may be changed instantly and uniformly by any desired amount by a stroke of the pressure pump. By changing the conditions in this way in the immediate neighborhood of a transition point, I have studied a considerable number of solid transitions under pressure. There is an essential

difference between a change from one solid to another and that from a solid or a vapor to a liquid. If a vapor and liquid or solid and liquid coexist, then at any definite temperature a single definite pressure of equilibrium will be automatically established. The ordinary kinetic mechanism makes this understandable. Equilibrium between vapor and liquid, for example, is maintained by the action of two streams of matter, one continually condensing on the liquid from the vapor, the other continually evaporating from the liquid into the vapor. When the velocity of the two processes is the same, we have equilibrium, and if equilibrium is disturbed, the two processes restore it. But with two solids, the behavior *may* be (although not necessarily) quite different, in that two solids may coexist indefinitely in contact at a definite temperature, anywhere within a range of pressure. This is different from the familiar persistence of a modification in a region of thermodynamic instability which involves viscosity. In such a system it is usually supposed that equilibrium will be automatically produced if the temperature is raised to the point of thermodynamic equilibrium and the two phases are in contact. The mechanism by which a definite equilibrium is automatically set up does not exist in the solid. In the solid it is probably near the truth to imagine the atoms as tied to certain mean positions of equilibrium and as vibrating about these positions, but never departing from them by more than a definite amount. In the liquid or vapor, on the other hand, we have something like Maxwell's distribution of velocities, and it is possible to find a few atoms with any velocity that we please, no matter how high above the mean. Suppose now two modifications of the solid to be in contact. If an atom is going to change from one modification to the other, it will have to free itself from its first position before it can settle down into the second. If the act of freeing itself from the first position involves too much of a departure from the mean, the atom will not be able to make the change, even if the new position, when once attained, is a position of greater stability But by a sufficiently large change of external conditions the atom may be helped to free itself from the first position, and so attain the second. I have studied in some detail the way in which the width of the "domain of indifference" varies with pressure and temperature, and also the variation of the velocity of transition outside the domain. Both show large and very different sorts of variation for different substances.

At present, we have absolutely no theory of polymorphism; we are acquiring by X-ray analysis a descriptive knowledge of the different atomic arrangements of polymorphs in some cases, but we have no explanation to offer of *why*, under some conditions, the atoms build themselves into one kind of structure and sometimes into another. For instance, who has any adequate explanation of why diamonds are so rare and graphite so common? There is here a curious example of the indirect progress that physics sometimes makes. It would seem to be a much simpler problem to explain the ways in which an atom may combine with others of its own kind than with those of a different kind, whereas in fact we have at least the beginnings of an understanding of a great many chemical compounds, but cannot claim anything of the kind for polymorphs. I cannot help feeling that an understanding of the great multiplicity of the phenomena of polymorphism under high pressures will be of assistance in understanding this fundamental matter, if only once somebody can find the loose end of the skein, and I even hope that it may be helpful in finding the loose end.

One particular transition produced by pressure is so interesting that I shall mention it in detail. Under proper conditions of pressure and temperature, ordinary yellow phosphorus is transformed into a black variety much like graphite in its properties, and of a density nearly 50 per cent. greater than that of the parent yellow phosphorus. The change is irreversible and permanent, and is the only example of such a permanent change produced by high pressure that I have found. The mechanism of the change is not at all understood, although a number of attempts have been made at explanation. The great difference of density points to something unusual. I wish to direct special attention to the conditions initiating the change from yellow to black. For a time varying from ten to thirty minutes before the change occurs, some preliminary change takes place throughout the entire interior of the mass of phosphorus, which is accompanied by a slight loss of volume, and which proceeds at an accelerated rate until the entire structure becomes unstable and collapses into the new modification. This preliminary change cannot be produced in the usual ways by external agents. For instance, we cannot inoculate a mass of yellow phosphorus with black and thereby either hasten or delay the transformation.

There is nothing else that I know of like this behavior. Certainly the ordinary formation of a new phase out of one that has become unstable by the formation of nuclei and the growth of these nuclei at a definite rate has nothing in common with this.

The electrical resistance of metallic elements in general decreases under pressure, the rate of decrease itself decreasing by a sort of law of diminishing returns as the pressure rises. The magnitude of the decrease under 12,000 kg. varies from 1 per cent. or less for metals like cobalt and tungsten to more than 70 per cent. for potassium and rubidium. There is no simple connection with the change of volume, as the change of resistance is of the order of ten-fold greater. The resistance of a few metals increases under pressure, and it is true of all of these that the rate of increase itself increases with the pressure. I have now, in addition, two unique metals: Cæsium, whose resistance at first decreases but later passes through a minimum and then increases; and antimony, whose resistance, at right angles to the trigonal axis, at first increases, but later passes through a maximum and then decreases. The effects of pressure are thus much more complicated than those of temperature (which are nearly the same for all metals), but they may, I believe, be even more suggestive in formulating a theory of resistance. Some important relations are brought out by the pressure tool that might not otherwise be known. Thus, the fact that pressure increases the resistance of solid bismuth, which is abnormal, but decreases that of the liquid, which is normal, strongly suggests that the abnormal behavior of solid bismuth is connected in some way with a peculiarity of the crystal structure. But, on the other hand, the fact that pressure increases the resistance of both solid and liquid lithium, shows that the abnormality is in some way more deep-seated for this metal, and is doubtless connected with some characteristic of the atom as a whole. The pressure effects bring out the fact that the conduction mechanism is not as simple as we might have supposed from the fact that the temperature coefficient of all metals is nearly the same.

There is, of course, no adequate theory of metallic conduction; the classical free electron theory has had to be given up, and there is no satisfactory substitute. I have myself made an attempt at finding the significance of the various pressure effects, and have been led by them to a very general conception of conduction which

I believe must be incorporated into the finally accepted theory. This conception is that the atoms play an essentially positive part in conduction and, in some way which we do not at present understand, make it possible for electrons to pass from one part of a metal to another. According to the classical conception, the rôle of the atoms was entirely secondary and negative; the atoms were the source of the free electrons, but, having once provided the electrons, their rôle was merely that of trouble-makers, getting in the way of the electrons and preventing them from moving about as they wished. But it now appears that the electrons cannot drift about without the intervention of the atoms. It is as if the atoms hand on the electrons from one to another when they are lined up in certain ways, or as if there were tracks between the intricate maze of quantum orbits within the atom, along which the conduction electrons may travel, so that there is an opportunity for long flights by the electrons when the tracks are properly aligned. All of the normal and abnormal effects of pressure are understandable in terms of a picture like this if we only suppose that the atom itself, when brought into close proximity to its fellows, behaves in the unsimple way that the facts of compressibility and polymorphic transition compel us to suppose. It is natural to suppose that these definite tracks are in some way connected with quantum conditions, and perhaps with high quantum numbers, but the details are too complicated for our working out at present.

The effects of pressure on thermal e.m.f. I have not attempted to incorporate into any definite theory, but they do suggest one important point of view. The pressure effects are complicated, there being maxima, minima, and complicated temperature effects. We do not in general expect that so simple a thing as a hydrostatic pressure will produce complicated effects, unless these effects involve a complicated mechanism. I have therefore drawn the conclusion that the thermal e.m.f. mechanism must be comparatively complicated, and not the simple thing supposed by the classical theory; this I have considered sufficient justification for not attempting to explain thermal e.m.f. in a first simple theory of the electrical properties of metals.

We pass now from the electrical properties to the thermal properties of metals. It is well known that there is a close relation between electrical and thermal conductivity, which is expressed

in the law of Wiedemann-Franz. The behavior of thermal conductivity under pressure shows that, although the relation may be close, there are other factors which have been insufficiently considered. The thermal conductivity of most metals increases under pressure, but that of some decreases, and for the most of them the change of electrical conductivity is greater than that of thermal. The reason for the difference is doubtless to be found in the contribution made to thermal conductivity by the atoms as distinguished from the electrons; but a consideration of numerical values suggests that the atomic part may be greater than has been supposed.

The thermal conductivity of minerals has been measured under pressure, a matter of importance to geophysicists. It has been found that in general the thermal conductivity of minerals increases under pressure, but the increases are not large enough to demand serious modification in our ordinary geological arguments, except possibly at great depths.

The effect of pressure on the thermal conductivity of liquids is interesting because it is so large; 12,000 kg. increases the conductivity by from two- to three-fold. In speculating as to the reasons for this, it appears that there is a very intimate connection with the velocity of sound. Entirely apart from the pressure effects, a reëxamination of the old facts with the new bias given by the pressure measurements has shown that the main mechanism of thermal conduction in a liquid is of surprising simplicity, being a sort of combination of the kinetic mechanism of a gas and the elastic wave mechanism of a crystalline solid. If we conceive of each molecule of the liquid as possessing the kinetic energy of temperature agitation demanded by kinetic theory, and handing this energy on to its neighbors with the velocity of an elastic wave of small dimensions—that is, with the velocity of sound—we shall account almost entirely for the thermal conductivity of ordinary liquids.

Apart from the various effects which have been measured accurately, results obtained under high pressures suggest other new points of view. Thus in the preliminary work, before I had found out how to make tight joints, or had learned what the capabilities of steel cylinders were in withstanding pressure, I observed a number of different types of rupture under unusual conditions, which are of some interest to the engineer. By select-

ing results under different conditions, it was possible to show that none of the ordinarily accepted criteria of rupture are valid except under restricted circumstances. The general, and therefore the significant, conditions of rupture are yet to be formulated.

Finally, we may glance at what remains to be done. Although the most readily obtainable results have been secured, the subject has not been more than begun. It becomes more and more impressed upon me that significant results are to be found in little frequented places, as for instance by working on materials that have extreme properties or in going to extreme temperatures. Thus, quite recently, this has led me to determine the properties of cæsium, the most compressible of the metals, with the extremely suggestive discovery of minimum resistance. The same sort of thing is to be done with many other materials of unusual properties. The extension of these high-pressure results to temperatures near the absolute zero, where phenomena assume an unwonted simplicity because of the absence of temperature agitation, will doubtless be of extreme importance. Results just as important lie also at high temperatures, a most difficult field, which the Geophysical Laboratory is skilfully attacking. Interesting special results may be found; the will-o'-the-wisp of manufactured diamonds is always before us. Without doubt, important practical developments lie in a field as yet practically untouched, that of organic, colloidal, and biological chemistry. It was shown a long while ago, at the West Virginia Agriculture Experiment Station, that high pressures will sterilize milk. I have found that high pressures in the cold will also coagulate egg-white, or the proteids of meat. Here is an enormous field untouched. We ought to know the effect of pressure on every substance of biological significance, just as we now know the effect of an elevation of temperature, and we may anticipate that important changes or combinations may be produced by pressure, just as they are now produced by temperature.

On the purely physical side, the field is immense. Entirely apart from new experimental knowledge, we cannot be satisfied with any theory which does not adequately explain the effects of high pressure already known, and conversely I venture to hope that these phenomena of high pressure may play an increasing part in formulating more adequate theories of the structure of matter.

SOME CAUSES OF VOLCANIC ACTIVITY.*

BY

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SEVERAL years ago I had the privilege of presenting to the Institute some conclusions respecting the sources of the energy through which the lava lake in the crater of the volcano Kilauea (Hawaii) was maintained in a fluid condition. The evidence then offered was obtained in association with E. S. Shepherd [1] from observations on the ground and in the laboratory, and may be reviewed somewhat briefly as follows:

Observations of the temperature of the lava lake, taken morning and evening and often at intervening hours of the day and night through a period of two months, revealed very considerable changes of temperature for so large a body of liquid magma. On June 13, 1912, for example, the surface temperature was 1070°. On July 6, 1912, it was 1185°. Temperatures as low as 950° have since been measured in the lava lake, and in its less active pools any temperature down to the point of actual solidification may be encountered. The lake in 1912 had the form of a single pool without partitions, differences of level, or islands, and was approximately 800 feet long by 500 feet wide. (Fig. 1.) The depth, of course, was problematical and not subject to direct observation except that the lava level rose and fell by small amounts daily, and observations over a period of years have shown maximum differences of about 700 feet; the containing basin must therefore have had a depth greater than this. In June, 1912, when these temperature observations were made, the lava stood about 200 feet below the rim and therefore some 500 feet above its lowest observed level during those years. The mass of lava contained in the basin was therefore very considerable, and a rise in its temperature from 1070° to 1185°, 115° within twenty-three days, must involve a very large and very rapid access of heat

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[1] ARTHUR L. DAY and E. S. SHEPHERD: "Water and Volcanic Activity," *Bull. Geol. Soc. Am.*, **24**, 573–606, 1913; Smithsonian Annual Report, 1913, pp. 275–305.

FIG. 1.

Lava lake in Kilauea Crater, May 23, 1912. A period of quiet overflow with but a few small fountains showing.

from some source below other than new lava, for the quantity of lava contained in the crater during this twenty-three-day interval under observation was substantially constant.

It is important to note further that islands are frequently present in the lake which have an upward and downward movement of different character and period from that of the fluid lava; also that when a fall of the fluid lava occurs, it often exposes what appears to be solid material only a few feet below the previous liquid surface, with no relation to the 700-foot level or to any deep-seated point. Such observations compel the admission that the material contained within the lava basin is considerably diversified in its physical state;[2] some is very fluid and some nearly or quite rigid.

This observation led to a rough laboratory test of some significance. On those occasions when the lake overflows its banks, which is a matter of daily occurrence during the rise of the lava column, there is visible movement of the fans and rivulets of new lava over the crater floor until they are so far cooled down that hardly a trace of red glow from them can be seen. The lowest temperature at which movement can be detected is therefore of the order of 600°. A fragment of this same lava brought to the laboratory and reheated will not flow under its own weight until a temperature in the neighborhood of 1300° has been reached. Such an observation is significant of a change of composition, and yet the only change of composition possible must be in the amount of volatile matter (gas) in solution in it. Gases are given off in abundance during cooling and their loss takes away from the lava its fluidity over a considerable range of temperature.

This observation, taken with the other, perhaps explains why the physical state of the lava in the basin is thus discovered to be partly rigid and partly fluid, partly porous and partly dense—its gas content is variable; and a further consequence is altogether probable, namely, that there are differences of temperature throughout the mass of material filling the crater, greater even than the observed differences at the surface.

Moreover, during this period of continuous observation in 1912, it was uniformly noticed that an increase in the amount of

[2] T. A. JAGGAR: "Seismometric Investigation of Hawaiian Lava Column," *Bull. Seism. Soc. Am.*, 10, 160, 1920.

FIG. 2.

Lava lake in Kilauea Crater, July 8, 1912, 7:30 P.M. More than 1100 fountains appear in this view.

gas discharged at the surface of the lake was always accompanied by a rise in surface temperature, and conversely during falling temperature the gas fountains diminished in number. A photograph taken at a time when the temperature was highest (July 3, 1912) offers a distinct record of more than 1100 fountains distributed almost uniformly over the surface of the lake (Fig. 2), while at other times no fountains at all are to be seen for intervals of a minute or more, and then only an occasional one appears. This observation immediately led us to the conclusion that there is a causal relation between the amount of gas discharged through the lava and the temperature of the fluid lava itself. The gases somehow contribute to the heat of the lava body.

One of the purposes of this expedition of 1912 was to collect gas from the lava basin at Kilauea if it should prove in any way practicable to do so. We were accordingly equipped with collecting tubes, pumps and pipe lines, but it was some days before a favorable opportunity offered. Just how fortunate we were in this collection we did not then appreciate, nor indeed for some years afterward, but it is probable that the gases then collected from a lava fountain on the crater floor, immediately adjacent to the lake itself, contained less evidence of contamination by atmospheric air than any volcano gases collected before or since. The conditions under which these gases were collected have been described before [3] and need receive no detailed attention at this time. They were studied in part by extemporized tests on the ground and in part through careful analysis in the laboratory some time later.

Two conspicuous features were revealed by these studies which bear intimately upon the subject of this paper and upon the study of volcanism in general, namely, (1) that the gas content of the different tubes filled at that time was widely different; (2) that the gases, whose composition we determined, could not be in equilibrium at the time and at the temperature of their escape from the lava. The first observation leads directly to the conclusion that the composition of each bubble which breaks through the surface, even from the same opening, is different; the second to the conclusion that the gases are necessarily in process of reaction at the time of their release. From our knowledge of gas reactions it follows, further, that gases still in process

[3] ARTHUR L. DAY and E. S. SHEPHERD: *Op. cit.*

of reaction at the time of release after passage up through the basin of liquid lava, must have been in process of reaction throughout their upward progress, that is to say, there must have been bubbles of gas of different composition uniting beneath the surface at frequent intervals during this excursion.

The analyses of the gases collected at that time revealed the presence of the following gases in different proportions in the collections: N_2, H_2O, CO_2, CO, SO_2, free H, free S, Cl, F and NH_3. Argon was also found after the first announcement was made. It is, of course, plain that free hydrogen cannot exist side by side with SO_2 or CO_2 at a temperature in the vicinity of 1000 degrees. Nor is free sulphur appropriately found in this group at 1100° C. Indeed, the fact that the proportions of these gases vary from tube to tube, when collected by continuous pumping from the same opening, plainly indicates that their relation cannot be a constant or a stable one.

Our knowledge of the reactions which must be taking place between these gases is adequate to establish the fact that heat is being contributed to the lava mass in consequence of these reactions going on within it. We have therefore certainly hit upon one of the sources of energy which serves to maintain the particular type of volcanism which is familiar to all at Kilauea. Very probably we have done somewhat more than this. If we have established the fact that continual reaction is going on during the passage of the gases through the liquid lava, whereby heat is contributed to the lava, it is a probable consequence that the temperature reaches its maximum at or near the surface. The consequences of this decision are somewhat far-reaching if one is minded to pursue them to their logical conclusion. For example, it appears to follow that the temperature in the basin will be lower as we go down into the lava, rather than higher, whence it is reasonable to suppose that the entire phenomenon is local and superficial rather than deep-seated.

Dr. T. A. Jaggar, Director of the Hawaiian Research Laboratory, has already confirmed this deduction regarding the temperature gradient by actual measurements of the lava temperature.[4] He used for this purpose a stout iron pipe, within which groups of seger cones were fastened at regular intervals, and

[4] T. A. JAGGAR: "Volcanologic Investigations at Kilauea," *Am. J. Sci.* (4), **44**, pp. 208 *et seq.*, 1917.

thrust it into the lava as far as he could (20 feet). Temperatures were found to be somewhat variable but lower by 100° or more than at the surface. The existence and behavior of the islands, and the heavy semi-rigid masses below the surface upon which they rest, also indicate a lower temperature below, for the circulation of gases in these masses will be considerably hampered. Very likely they are themselves losing gas more or less continually and thereby losing their fluidity. In any event, such a temperature distribution does not afford support for the old hypothesis that volcanoes are "safety-valves" to insure the stability of a molten interior, indeed they afford not the slightest indication of the existence of a generally molten interior.

In a book which appeared but a few days ago, Mr. Harold Jeffreys, of Cambridge University, offered a careful analysis of existing evidence of the manner of formation of the earth [5] and the probable state of its interior. His reasoning has seemed to me to include more of modern physics and chemistry than the reasoning hitherto applied to the constitution of the earth. He figures that the gaseous mass, presumably split off from the sun by tidal rupture due to another heavenly body passing too near, condensed within a comparatively short time to a liquid nucleus. This nucleus in its normal convection cooling then acquired a crust which broke up and sank as soon as formed, its specific gravity being measurably greater in the solid than in the liquid state. It is no more than a generation since distinguished physicists were still in doubt about the relative density of solid and liquid lava, and some then believed that the crust ought to float as readily upon the molten interior of the earth as patches of crust float upon a liquid lava basin, like that at Kilauea (Fig. 3). Closer observation, however, has since established the fact that the floating is but temporary and is due to nothing more than the accident of gas accumulations beneath these patches of crust, which give sufficient buoyancy to support them on the surface for a long time, but only for so long as they remain perfectly poised. The rise of a bubble more violent than the rest, which is sufficient to tilt one of the blocks but a very little, or an overlapping wave in the lava surface, will quickly cause them to

[5] HAROLD JEFFREYS: "The Earth, Its Origin, History and Physical Constitution," Cambridge, 1924.

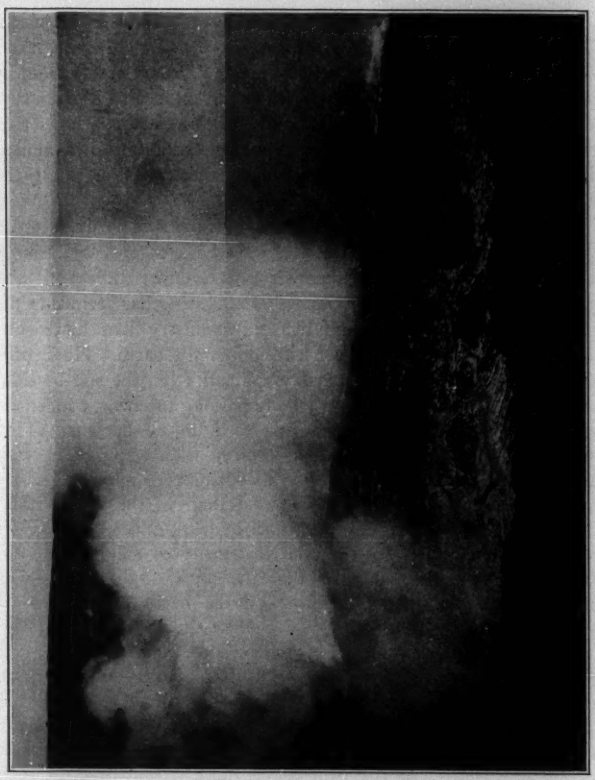

FIG. 3.

June 8, 1912. Lava lake in Kilauea Crater showing large sheets of floating crust.

lose the supporting gases and to glide out of sight beneath the surface, much as a piece of sheet iron sinks in water.

The normal behavior of a crust forming upon a fluid mass highly charged with gas, such as we must believe the nucleus of the earth to have been, is to sink, and so Jeffreys concludes that though the first blocks to sink were undoubtedly remelted presently in the course of their downward movement, and so merely helped to cool the magma, eventually the sinking crust accumulated at the centre and the resulting earth became in consequence something of a honeycomb structure, the rigidity of which may quite properly be equal to that of steel, as has been so completely shown by the tidal studies of Michelson and Sir George Darwin, and that any portions remaining liquid can be nothing more than the product of local conditions which presumably are slowly passing away.

This concept of the earth as a whole readily lends itself to the interpretation of volcanoes as we find them. It is extremely difficult to conceive of a volcano as the safety valve of a molten interior, as the older geologists were accustomed to think of it, or of a molten layer concentric with the crust as it was later conceived to be, so long as two such openings continue to exist side by side in the Island of Hawaii, one of which (Mauna Loa) is 10,000 feet higher than the other (Kilauea) and yet liquid lava emerges more frequently and in greater quantity from the higher opening, or its subsidiaries, than from the lower. It is also a matter of record that greatly increased activity in the one basin rarely finds an echo in the other. The liquid lava of Hawaii is at least two and one-half times heavier than water. If these two outlets were connected with a common liquid interior, the lava column in the higher conduit would be pressing for discharge at the lower opening, not at the higher, and with a pressure of the order of magnitude of a thousand atmospheres upon every square inch of its area. Similarly the volcanoes of Central America and of the Alaskan peninsula maintain each its independent behavior apparently regardless of what is happening to its neighbors. So far as the record of field observations goes, therefore, the number of simultaneous eruptions which have occurred in neighboring volcanoes in recent time is so small as to leave little room for any other explanation than coincidence.

If for these several reasons we limit our conception of a volcano to a purely local phenomenon arising from unusual local conditions, and if we may assume that other observations above outlined offer reasonable ground for the supposition that gases, reacting among themselves, contribute materially to the heat necessary to maintain a small lava basin like that at Kilauea in a liquid state, then we have two further steps to take in order to give to this concept a tangible and reasonably independent existence. First, where may such an extraordinary phenomenon as a volcano be supposed to take its origin, and second, by what means is the indispensable heterogeneity among the gases maintained in consequence of which the reactions occur? Obviously this is reasoning somewhat beyond any phenomena actually observed, but the underlying region beneath a volcano will perhaps never be accessible to direct observation so that inference from the best experience we have must be drawn upon to complete the picture.

Before attempting to indicate the direction in which this search leads, it will be helpful to consider the phenomena at another volcanic centre which was active between 1914 and 1917. I refer to Lassen Peak in California. This volcano is entirely different in its behavior from Kilauea. Instead of an open basin of liquid lava, more or less continuously active but rarely overflowing, here is an explosive volcano which emitted no liquid lava and only once during its four years of activity reached the temperature indicated by red heat.

The outbreak at Lassen Peak began with an explosion in the summit crater in early spring (May 30, 1914), the first outburst breaking through snow of considerable depth. These explosions continued at intervals of four or five days throughout the summer and autumn, possibly to some extent in the winter season also, but Lassen Peak is over 10,000 feet high and the summit during the winter season is shrouded in cloud. In May of 1915, almost exactly one year after the initial explosion, came three days of terrific activity, during which the dust cloud reached a height of 25,000 feet above the summit of the mountain, and blocks the size of a man's hand were thrown for ten miles. During this culminating outbreak, or just before it, the old volcano plug forming the floor of the crater was raised 300 feet or more to a position level with the crater rim, the force behind it, apparently, being

just insufficient to blow it completely off the mountain and so to expose the volcano hearth. The point of greatest weakness then proved to be at the northeast side of the inclosing cone, somewhat below the summit, and two devastating, horizontal explosions, the latter concluding the period of most violent activity, found vent through this opening. Although the adjacent valleys of Hat Creek and Lost Creek were stripped of all vegetation for a distance of more than four miles by these horizontal blasts (Fig. 4), no fire appears to have been set by the explosions, save momentarily on the face of a steep cliff directly exposed to the second blast, and even this was confined to a small area of dead leaves and twigs which supported combustion for no more than a few moments. Red-hot ejecta at the summit or illuminated smoke clouds at night were seen only once (May 19, 1915), and then the color was described as "deep-red," indicating a temperature not higher than 750°. Generally the ejecta which were thickly strewn about the country adjacent to the crater were not warm enough to melt the snow on which they fell, and even when of great size they broke through the snow without melting it away. In mid-summer likewise no case is known where the ejecta were hot enough to kindle a forest fire, although a careful watch was kept by the Forest Ranger Service through four summers for just such a contingency.

Following this outburst the eruptive activity gradually subsided, the year 1916 offering but a few mild explosions and 1917 only a short concluding outbreak (also in May), of considerable violence while it lasted, but not comparable either in volume or intensity with the great outbreak of May, 1915.[6]

This brief outline of the character of the eruption of Lassen Peak is sufficient to show that in spite of the violence of the explosions, no evidence of the high temperatures of Kilauea or Vesuvius[7] appeared at the surface during the entire period of activity. There were steam explosions of tremendous volume and power, but no local development of heat by chemical activity, such

[6] For further details and laboratory studies see ARTHUR L. DAY and E. T. ALLEN: "The Volcanic Activity and Hot Springs of Lassen Peak," Carnegie Inst. of Washington, Publication No. 360, 1925.

[7] For an account of activity of Vesuvius in recent years see F. A. PERRET: "The Vesuvius Eruption of 1906, Study of a Volcanic Cycle," Carnegie Inst. of Wash., Publication No. 339, 1924.

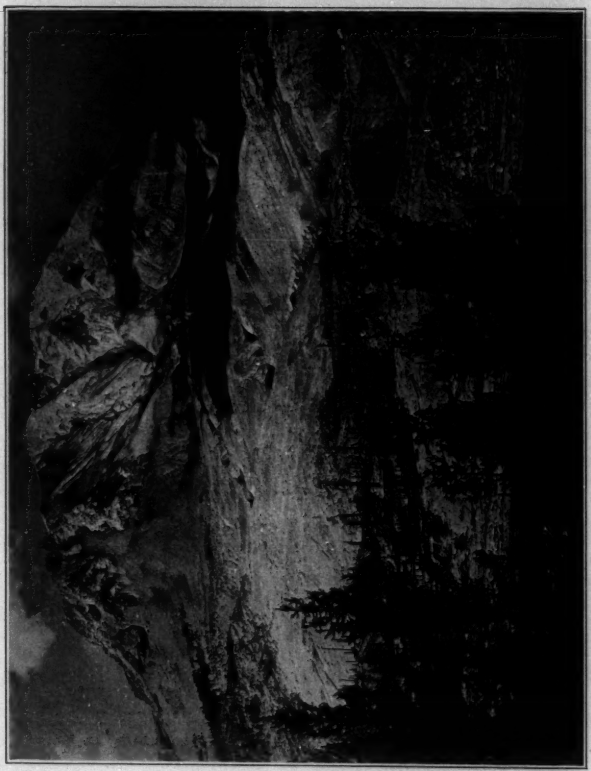

FIG. 4.

Lassen Peak from the northeast after the great eruption of May 22, 1915. The foreground for a distance of four miles from the mountain was virgin forest reserve before the eruption.

as has been described in the case of Kilauea, was detected at any time.

In considering this case, it is appropriate to emphasize the fact that there has been no other eruption of this volcano since the region was inhabited by white men, and geological conditions indicate that at least 200 years have elapsed since the last show of activity.

If a volcano is assumed to be a slowly cooling system maintained by residual heat from a local hot zone below the surface, then what manner of mechanism is it which can account for the sudden development in such a cooling system of violent explosive activity sufficient in volume to extend over several years, and of such intensity as to cover the surrounding country with boulders of considerable size, without displaying evidence of high initial temperature or contributory chemical reactions?

It has been a long search to find an explanation which appeared properly to account for an outbreak of this kind, but a suggestion has at last been found, partly as a result of theoretical reasoning and partly from experience in the laboratory, which appears to account for it completely and at the same time to go far toward elucidating the entire problem of volcanism.[8] Freed from chemical terminology, the mechanism is simply this: A silicate solution in its liquid state can take up water in solution in considerable quantity. A simple solution of silica and potash when heated under pressure in the laboratory is capable of taking up as much as 12.5 per cent. of water in solution. A rock magma in the earth is just such a silicate solution, although more complicated in character, and is entirely competent to carry 5 or 6 per cent. of water in solution under appropriate conditions.[9] If it should happen that the lava beneath a volcano carries such quantities of water in solution, then all the phenomena of volcanism become appreciably clearer, for in the 8000 or more analyses of crystalline rocks of igneous origin which have been gathered together by Washington and published by the U. S. Geological Survey[10]

[8] G. W. MOREY: "The Development of Pressure in Magmas as a Result of Crystallization," *Jour. Wash. Acad.*, **12**, 219, 1922; ARTHUR L. DAY and E. T. ALLEN: *Op. cit.*

[9] *Cf.* J. W. JUDD *et al.*: "The Eruption of Krakatoa and Subsequent Phenomena," p. 36 (Rept. of Krakatoa Com. Roy. Soc. London, 1888).

[10] H. S. WASHINGTON: "Chemical Analyses of Igneous Rocks, Etc." U. S. Geol. Surv., Prof. Paper No. 99, 1917.

there is none containing more than 1½ per cent. of water and less than 1 per cent. is usual. This must mean that in the process of crystallization of the rock from the magma the water content is for the most part discharged. Should it happen that this discharge of water takes place in a closed space, then immense pressures might develop and explosive activity of tremendous intensity might result. Furthermore, if the amount of participating magma were large and the rate of crystallization considerable, such activity might continue for long periods of time.

Beneath the surface, magma basins are, of course, inaccessible to direct observation, but two facts have been noted in the study of volcanoes which tend to confirm this reasoning. It has already been stated that the lava lake at Kilauea has a temperature, usually, between 1000° and 1200°, and fluid lava overflowing the rim of the basin at that temperature still remains fluid at 700° or 800° (red heat). This is, of course, representative magma or lava which is in process of crystallization. If samples of this lava be collected after solidification and reheated in the laboratory, they cannot be brought again to molten condition below 1300°. It is, therefore, clear that the fluidity of the lava in the basin is due primarily to the volatile ingredients which it contained and has discharged during crystallization, of which water is the chief. In complete accord with this, the great outbursts of gas and dust-laden steam from the more violent volcanoes, like Vesuvius or Lassen Peak, are nothing other than this volatile content of the magma below, discharged explosively.

It sometimes happens that volcanic magma, either through peculiarities of chemical composition or through sudden exposure, whereby it is quickly chilled, solidifies without crystallization. Such volcanic glasses are commonly called pitch-stones or obsidians. A very considerable mass of such obsidian is familiar to all tourists in the Obsidian Cliff at Yellowstone Park. In smaller quantities obsidian is frequently found in nearly all volcanic regions. Such uncrystallized volcanic magma when analyzed is often found to have retained much more of its water content than the 1½ per cent. maximum which has just been cited for crystalline rocks.[11] Here, therefore, is direct confirmation, *i.e.*, as direct as the inaccessible character of the subject is ever likely to permit, of the conclusion that the water content of magma before crystal-

[11] *Cf.* J. W. JUDD: *Op. cit.*

lization is greater than the water content of the rock which forms from it. During the process of crystallization, therefore, this water must in some manner be discharged.

If we apply these observations to Lassen Peak and suppose a body of magma in process of slow crystallization at relatively low temperature to exist beneath the mountain, and suppose further that the water set free during the crystallization process is maintained under pressure in a closed chamber, there is danger of a violent outbreak whenever a structural weakness develops in the containing vessel. Lassen Peak is situated in a region often visited by earthquakes, it therefore requires no violent stretch of the imagination to believe that one of these earthquakes did, in fact, weaken the structure early in 1914, and that in consequence of this a small explosion through the floor of the summit crater occurred. More or less in confirmation of this supposition the forest rangers who first visited the mountain on the day following the explosion noted two cracks extending east and west from the explosion opening for a hundred feet or more. The explosion itself, so far as may be gathered from witnesses, was wholly inadequate in volume to split open the mountain, and indeed the small crater formed by the explosion is reported to have been no more than 20 × 40 ft. and extended only a few feet into the loose, scoriaceous material covering the crater floor.

This discharge of steam in violent explosion was followed by similar outbreaks at intervals of three or four days during the next seven or eight months, the periodicity being determined no doubt by the rate of crystallization and the amount of the overlying load. In general, the volume and intensity of the successive explosions increased as they proceeded, which might reasonably be expected as more and more of the crystallizing mass came to feel the release of pressure after the opening had been made.

The laboratory study of such solutions indicates a further possibility through which the early stages of such an outbreak might be expedited. The fact that even when the explosions had reached their maximum intensity there was no development of high temperature, suggests that the magma below may itself have been at a relatively low temperature, ultra-viscous and inert through undercooling; that is to say, being inclosed within an impervious containing vessel at comparatively low temperature.

crystallization had slowed down until considerable portions were cooled below the temperature of normal crystallization while still in the vitreous state. It is, of course, well known that silicate solutions (*cf.* window glass) are prone to crystallize with extreme slowness, otherwise probably we should have no glass industry. In such an inert mass, therefore, in which crystallization was proceeding thus slowly, the advent of water vapor from without would have the effect of diminishing the viscosity and increasing the rate of crystallization.

Following the first explosion at the summit of Lassen Peak, it was noted that the great body of accumulated snow within the summit crater was rapidly melting and flowing into the explosion cracks. This water must have found its way to the volcano hearth at first by percolation and later, within the hot zone, as steam. We know from laboratory experiment that steam is readily soluble in such an undercooled silicate magma, with the effect of increasing its fluidity and thereby precipitating crystallization or increasing the rate of crystallization already going on. The operation of crystallization itself, as has been shown above, sets free more water so that the operation becomes more or less self-perpetuating so long as any considerable amount of magma remains uncrystallized within reach of the outlet channels (reduced pressure). This explanation appears adequate to account both for the initiation and the progress of explosive volcanic activity at Lassen Peak during the period of 1914 to 1917. It also indicates the source of the vast amount of water vapor which is given off in the course of a long series of explosive eruptions.

In this discussion of the phenomena at Lassen Peak we appear to have reached an answer to one of the two questions posed earlier in this address, namely, the point of probable origin of the gases which participate in volcanic activity. They are in solution in the crystallizing magma and their release takes place under appropriate conditions of temperature and pressure when this magma crystallizes to rock. Chief among these volatile ingredients is undoubtedly water in most, if not in all, cases. If water be substantially the only ingredient, as at Lassen Peak, then the volcanic outbreak will consist of discharges of dust-laden steam and such solid material as may be carried out by attrition. If temperatures are higher and the volatile ingredients include

chemically active gases such as chlorine, fluorine, sulphur, hydrogen and the like, then an accelerated type of development may be expected, due to the higher temperatures resulting from the combination of these gases when oxidized either through reactions within the magma or with oxygen from the air. Under such conditions the magma may be forced out in liquid condition (lava flow) and crystallize in the open air.

After observing the persistence of the open lava lake and noting, as Professor Jaggar has been able to do by measurement, that the temperature at the surface is higher than it is immediately beneath the surface, it again requires but little of the imagination to enable one to understand how such gas reactions may provide the necessary energy to maintain the lake in fluid condition. The origin of the gases here, as in the case of Lassen Peak, must be sought in crystallizing magma at different depths where differences of temperature and pressure must be responsible for very considerable inhomogeneities (differences of concentration) in the gases from different local sources.

The Kilauea crater is not appropriately pictured as an ordinary boiling flask with a long, narrow neck. It is very doubtful if we have any ground for such a picture. At any rate, during the past summer (1924), when the lava lake had been drained away through subterranean channels and the gases were escaping explosively from the empty basin, the crater was enlarged by the explosions to about 3500 feet in diameter and 1500 feet in depth. The volcanic throat was thus laid open to a depth more than twice as great as on any previous occasion since it came under the observation of white men, and a rare and precious opportunity was offered to see what manner of material occupied the next 800 feet immediately under the lava lake which has so long filled this throat to varying depths from a few feet to as much as 700 feet or more.

To those who have thought of the volcano crater as a downward-extending pipe or conduit through which the lavas of centuries have been quietly poured out upon the surrounding terrane, the appearance of the enlarged and deepened opening must have been an unexpected revelation. It appeared quite dry—no freshly cooled volcanic glass (obsidian) or partially crystallized lava was visible below, nor was any found among the fragments thrown

out (Fig. 5) by the explosions. The lava fragments were from older layers than any before exposed at Kilauea, completely crystallized, nearly free from bubbles, rather fine grained, though rich in olivine and feldspar phenocrysts like the lavas from its great neighbor, Mauna Loa, but in physical appearance totally unlike the Kilauea flows which have almost come to be regarded among geologists as a fixed type.

At one side of the great bowl, about 600 feet above the bottom, was an area some 500 feet in its transverse dimension and more

FIG. 5.

Kilauea Crater. Rocks thrown out by the violent eruption of May, 1924.

than 100 feet thick, which showed here and there a trace of red at night. The surface also had the appearance of fresh Aa lava, which occasionally flaked off in considerable areas, revealing a bright red patch beneath. Above it on the rim a hot air-current was continually depositing fine flakes of freshly oxidized, iron-bearing scale. This may have been one of the feeders of the lava lake. It was certainly the hottest spot left exposed in the empty basin. Another smaller area in one corner of the bottom was distinguished by a half-dozen roaring gas outlets whose throats glowed red at night. This may have been a smaller feeder. When the lava began to return to the pit in July, it spouted out from a point high up on the talus pile on the opposite side of the

basin in a fountain 175 feet high. This must have been from a third feeder. No others have so far been discovered.

Instead of the boiling-flask picture, therefore, we should think rather of a central collecting tube, with many more or less wide-spreading branches below, leading to local chambers in which crystallization is proceeding under different conditions of temperature and pressure. That a number of such branches exist is only discovered on a rare occasion like the present summer when violent explosions have cleaned out the lava basin and enlarged it to many times its former volume. Then it is seen that the openings leading into and out from it are on the side walls while the bottom is to all appearance solid, old and (relatively) cold. Even the solid ejecta which were thrown out during this period of violent activity include no obsidian masses and no lava as recent as that represented in the lava lake and in the flows of recent years.

That the gases from these different sources differ in composition is abundantly shown by their continual reaction when they meet in the lava lake, as has been explained in the earlier part of this address.

That the fluid material in these individual pockets is under different pressures as well as different temperatures is also shown by many observations of small streams of lava which emerge from the talus pile, often 100 feet or more above the level of the lava lake, and flow down into it (Fig. 6). Such a flow observed in June, 1912, lasted for several days and released a very considerable body of lava.

If the lava lake represented the outlet of a single subterranean magma chamber with which it is supposed to be connected through a single neck or funnel, then the establishment of a second conduit from one to the other will not be competent to deliver lava at a higher level than the first, or gases of different composition. A separate chamber and an additional source of energy are necessary for that. Thus it has happened that the heterogeneous character of the gases collected, the temperature conditions within the lava basin, the appearance of the crater when free of all its liquid lava and the dynamic relations within the lava body when present in the crater, all point to many sources rather than to a single source both of gases and of magma.

An interesting sidelight of quite another kind has lately been thrown on this problem from an entirely unexpected source. In

FIG. 6.

May 23, 1912. Lava lake in Kilauea Crater showing a fresh outpouring of lava from the talus high above the level of the lake.

the month of July, 1919, Professor Jagger took advantage of a favorable opportunity to make a continuous series of observations, extending through the entire month, of the level of lava in the basin of Kilauea. The observations were made by vertical triangulation from datum points on the rim of the pit at intervals of twenty minutes, day and night, with the purpose of ascertaining whether or not a lunar or solar influence upon the lava mass could be established. The examination of the data was made by E. W. Brown,[12] than whom there is no one with wider experience in the discussion of tidal phenomena, and although the curves at first sight appeared to be of periodic character and were so regarded by Jaggar, no more than an inch or two in apparent changes of several feet could be attributed by Brown to luni-solar influence. Had the magma chamber been very large, an unmistakable tidal effect would certainly have been found.

And so we return from this investigation also to the conclusion that volcanoes are local and superficial developments representing (geologically) the last stages of crystallization in a mass of magma below, of which little remains fluid, and this in small (as geologic dimensions go) pockets, which are quite variable in gas content, pressure, and conditions of chemical equilibrium.

There were times, even in late geologic history, when quite different conditions prevailed, when masses of fluid magma were larger and poured out through crustal rifts several miles in length without signs of explosive activity or any restraint upon complete freedom of crystallization and release of their volatile content. Such conditions gave us the successive flows known as the Deccan traps, the Stormberg lavas of South Africa and the great basaltic outflows of the Snake River basin and adjacent territory in this country which covered nearly 250,000 square miles. Latest of these and appropriately smallest in extent is the flow from the Laki rift in Iceland in 1783. It may well be that such outpourings are past. It is a rare volcanic outburst in modern times which yields as much as a cubic mile of lava. Matavanu in Somoa is reported to have released this quantity in a continuous flow through the six years ending in 1912.

Through all of these studies our conclusion seems to stand fast wherever it is applied, namely, that the outstanding factor in

[12] ERNEST W. BROWN: "Tidal Oscillations in Halemaumau, the Lava Pit of Kilauea," *Am. Jour. Sci.* (5), **9**, 95, 1925.

determining the character of modern volcanism is the gas content of the crystallizing magma. If this be mainly of steam released in a closed chamber, as at Lassen Peak, then only steam explosions are to be expected as the surface manifestation of the crystallization of the magma below; if to the steam are added such chemically active gases as chlorine, sulphur, hydrogen and the hydrocarbons, then chemical reaction between these will be a sufficient cause of higher temperatures and lava flows of the character well known at Vesuvius, Stromboli or Kilauea.

The Origin of the Spectrum of the Aurora. L. VEGARD, H. KAMERLINGH ONNES and W. H. KEESON. (*Comptes Rendus*, April 6, 1925.)—Solid nitrogen and mixtures of nitrogen and neon were bombarded with rapid cathode rays at the temperature of liquid helium. The band designated N_1 by Vegard and obtained from solid nitrogen is just about the same at the temperature of liquid helium as at that of liquid hydrogen. The experiments with solid neon containing different proportions of solid nitrogen show that the maximum of this band moves toward the long wave-lengths as the proportion of nitrogen in the mixture grows less, and that the same is true for a mixture of nitrogen and argon. The authors claim that in the limit and in a mixture of solid neon and nitrogen the N_1 band turns into a single line of wave-length 5576.6 Å. and that this about coincides with the green line of the aurora. They attribute the discrepancy between this and the actual wave-length of the auroral line, 5577.+ to some influence of the neon present. It must be confessed that this claim does not carry conviction such as is felt after reading the recent identification of this line with one obtained in Toronto by passing the current through mixtures of air or oxygen with helium at low temperatures. G. F. S.

New Test for Copper, Iron, and Cobalt.—M. L. NICHOLS and S. R. COOPER, of Cornell University (*Jour. Am. Chem. Soc.*, 1925, 47, 1268–1270), have found in dinitroresorcinol a new reagent for copper, iron, and cobalt in qualitative analysis. The reagent is used as a freshly prepared, hot, aqueous solution, which is clear and has a brown color. In the presence of sodium acetate, this solution yields a dark brown color or a brown precipitate with cupric salts, a green color or precipitate with ferric salts, and an orange-red color or precipitate with cobaltous salts. This reaction will detect the presence of the following amounts of each metal in 1 c.c. of the solutions of their respective salts: Copper, 0.0040 milligram; iron, 0.0035 milligram; cobalt, 0.0033 milligram. Cadmium and aluminum salts do not interfere with the reaction; but salts of chromium and nickel may yield colors or precipitates. J. S. H.

THE PRINCIPLE OF RELATIVITY.*

BY

W. F. G. SWANN, D.Sc.

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Two years ago, the physicist whose interests tended towards the mathematical side went about in continual fear of the enthusiastic but non-scientific seeker after wisdom who would ask him to "explain the theory of relativity," and who would betray his reluctance to hard thinking by calling for the explanation "in a few words." A colleague once asked me how to get rid of such a man. I replied, "Start to explain the theory of relativity to him." Now while I should be sincerely chagrined to see the consequence which I there anticipated materialize on the present occasion, one has to face the fact that the subject is not one which lends itself readily to our imagination.

To appeal immediately to our imagination, a theory must speak in a language to which we have been accustomed. It must accept dogmas and intuitions which our previous thinking has ingrained in us, and it must build us a picture in terms of these. Now one of the characteristic features of the theory of relativity is its insistence that we trace to their logical foundations these very dogmas and intuitions, particularly those pertaining to space and time; and, in doing this, it disturbs our equanimity with regard to them. It shows us that, in order to speak with consistency about nature, we must remove from our minds certain ideas about space and time which ought never to have been there, and leave only as much as is justified by the facts. Unfortunately, these ideas which we are asked to remove are among those which have impressed us most, so that we are left as children who, viewing the hotel porter stripped of his medals and gold braid, are unwilling to believe that anything about him which is tangible remains, or that he can any longer be capable of performing his functions. But, you may say, "Why strip the porter of his medals if they were a comfort to us?" Well, the trouble is that we had got into a stage in our thinking in which we had become so impressed by the medals and the gold braid that we believed

* Presented at a meeting held Thursday, March 12, 1925.

that no porter could be a porter at all unless he wore the uniform of our establishment. Ours was a real porter. Those in the other hotels were only makeshifts unconsciously deceiving the patrons of the place as to their qualifications for portership by doing everything that a porter should do. Now we might have continued to enjoy the satisfaction of this viewpoint had we not entered into conversation with one of the guests in the other hotel and found that he was as convinced of the authenticity of his porter as we were of ours, and not only this, but that he was able to claim for the action of his man everything that we could claim for ours. With this state of affairs, we are brought to the sad necessity of asking ourselves what a porter is anyhow, and are reluctantly compelled to admit that the gold braid is not an essential feature. And so, as we shall see, it will turn out that some of those things which we have thought about space and time are as the medals and gold braid of our porter.

The theory of relativity had its origin in attempts to determine what was spoken of as the earth's absolute motion in space. There is no difficulty in determining our motion relative to one of the stars, for example; but, that tells us nothing about the earth's absolute motion if we do not know the velocity of the star. The question which arose was this: Suppose that we shut ourselves up in our laboratory so that we cannot see outside. Is there any possible experiment which we can do in that laboratory which will tell us whether we are or are not in uniform rectilinear motion through space, and if we are in motion, what our velocity is?

I can perhaps illustrate some of the ideas involved by an appeal to Fig. 1. Suppose that S represents a boat which can sail along a river in the direction of the arrow. Suppose that to the prow of the boat we attach two poles of equal length, one parallel to the length of the boat, and the other perpendicular thereto, and suppose that to the end of each pole we attach a board which dips into the water. If we drop a stone from the prow of the boat into the water, ripples will travel out, will be reflected by the boards and will return. If the boat is at rest with reference to the river, the waves will return in step in view of the equality in the length of the poles. If, however, the boat is in motion in the direction of the arrow, the time taken for a wavelet to travel from the prow of the boat to the board A will be increased, because the board

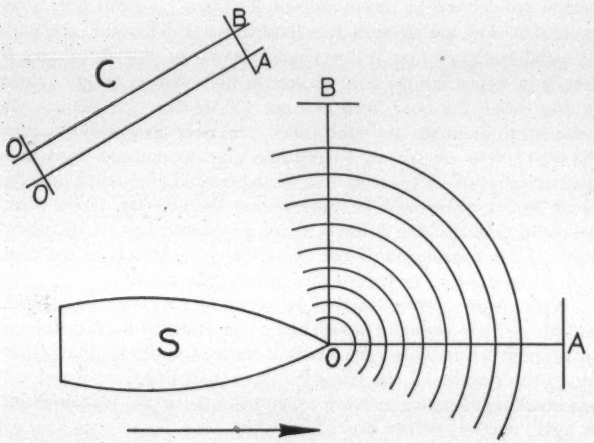
will move on as the waves travel towards it. On the other hand, the time of the return journey will be shortened, since the boat advances to meet the returning wave. It is a comparatively easy matter to show that the loss of time on the outward journey of the ripples is greater than the gain of time on the return journey, so that, on the whole, the time taken by a ripple to leave the prow of the boat, strike the board A and return, is increased by the motion of the boat. It is further a matter of simple calculation to show that the waves which travel out to the board B and return are delayed by the motion of the boat. It turns out, however, that they are delayed less than those which travel the path OB and back, so that if the ripples from the boards A and B return in step when the boat is at rest, they will no longer return in step when the boat is in motion. Moreover, if, keeping the poles at right angles to each other, we were gradually to turn the boat across stream, we should find that the amounts by which the two pairs of waves were out of step would alter; and, indeed, if we had no other way of determining the velocity of the boat, we could calculate that velocity from a measurement of the alteration of the retardation of the two trains of wavelets as the boat with its two poles was turned through a right angle.

Now, using mirrors and light waves, Professors Michelson and Morley performed an experiment exactly analogous to this in an attempt to measure the earth's velocity through that ether which the physics of the last fifty years had urged us to regard as pervading all space, serving as a medium for the transmission of light, wireless waves, and gravitation.

The result of the experiment was just what might have been anticipated if the earth had been at rest in the ether. No retardation of one set of waves over the other was found. The assumption that the earth is at rest at all times of the year is, however, untenable since it moves around the sun. If in the case of experiment with the boat we had found a similar result and had been unwillng to assume that the boat was at rest, we could have got over the difficulty by supposing that the river was in motion, so that it carried the waves along with it, and that there was no relative motion between the boat and the river, the former simply drifting along with the latter. From an astronomical point of view, however, there are grave difficulties in supposing an ether which moves along with the earth, difficulties so great

as practically to rule this possibility out of court. If in the case of a corresponding result with the boat we had been unwilling to admit that the stream was in motion, there would be only one way out of the dilemma. We could say that the anticipated loss of time by the waves which travelled the distance OA and back was just compensated by the pole OA being slightly shorter than the pole OB. You will answer, however, that we made these poles of equal length. I reply to this by inquiring just what

FIG. 1.



we did when we made them of equal length. We set them side by side, as at C (Fig. 1); and then took a saw and cut right across the two at two places. Suppose, however, that on turning the rod OA from the direction indicated at C to one parallel to the direction of the velocity it became slightly shortened, and on turning the rod OB until its length was perpendicular to the velocity it became slightly lengthened, then the poles when attached to the boat in the manner shown in the figure would no longer be of equal length. If everything participated in this shortening and lengthening we should have no way of becoming conscious of it. We could not ascertain the difference in length of the two rods OA and OB by means of a measuring

scale because the scale would itself experience change in its dimensions as it was turned around.

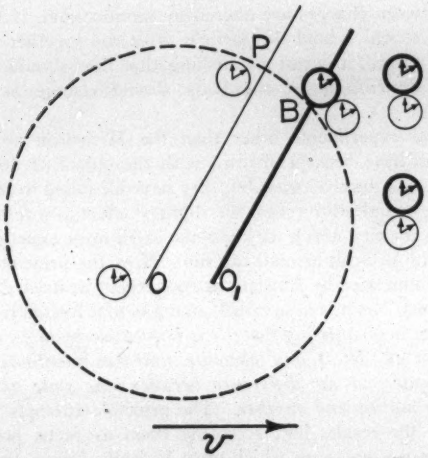
Such then was the assumption made by Fitzgerald and Lorentz to account for the Michelson and Morley experiment, the assumption that the rod is shorter when its length is parallel to the line of motion than when it is perpendicular thereto. Drastic as such an assumption may appear at first sight, it becomes much less artificial when taken in the light of modern views as to the electrical constitution of matter. For we know that the electric forces between charges are altered by motion; and, if the forces of cohesion which hold the particles of a rod together are of an electrical nature, it is not astonishing that they should alter, and that the dimensions of the body should change as a result of motion.

Several experiments other than the Michelson and Morley experiment have been performed with the object of determining the earth's absolute velocity, but they have all failed to reveal such a velocity, or indeed to reveal the slightest effect as a result of that change in velocity which we know the earth must experience since it moves in an orbit around the sun. Now the principle of relativity as enunciated by Einstein in 1905, the "Restricted Principle of Relativity" as it is now called, amounts to a formal recognition of this fact in postulating that: *It is impossible by any conceivable experiment to detect any absolute uniform rectilinear motion through space, or the difference between one state of uniform rectilinear motion and another.* The principle attempts no explanation of the results but, accepting them as facts, proceeds to deduce the consequences which must logically follow from them. It is these consequences which give the theory of relativity some of its peculiarities.

In particular, the theory of relativity requires that the velocity of light as measured by all observers shall be the same, whatever the direction of the light, and whatever the state of motion of the observers, so long as the motion is uniform and rectilinear. In other words, if we have two observers, one of whom we shall speak of arbitrarily as at rest, and another whom we shall speak of as in motion, both of these observers must measure the same value for the velocity of light. Now let us see what this means. Suppose that the stationary observer, whose apparatus I shall indicate by light lines, sets up a number of clocks at different

points of space, and provides himself with a long scale with which to measure distances. Suppose that he sends out a light signal from the point O (Fig. 2) at a time T_1 as recorded by his clock at O and notes the time T_2 of its arrival at the point P as recorded by another of his clocks situated there. Then, in order to calculate the velocity of light, he will divide the length L as recorded by his rod, by the time difference $T_2 - T_1$. Now what will the moving observer, whose apparatus is indicated by heavy lines, say

FIG. 2.



about the matter? Let us first trace the consequences of assuming that any one of the moving observer's clocks and scales always agrees in reading with any clock or scale of the fixed observer with which it may happen to coincide momentarily. If his apparatus happens to coincide momentarily with that of the fixed observer when the light signal is started, the moving observer will agree that the signal was started at the time T_1. By the time the light-wave has reached the point P on the fixed scale, however, the moving scale will have moved to the position shown by O_1B and the reading of the wave on it will be less than on the fixed scale. We may suppose that the moving observer had a clock at this point ready to record the time of arrival of the light-wave

there, and he will of course record this as T_2, so that the time interval will be $T_2 - T_1$ as before. If therefore the moving observer's clocks and scales read in a way which agrees with that of the fixed observer's clocks and scales, it should turn out that, if the moving observer calculates the velocity of light by dividing the difference of the two readings on his scale by the time $T_2 - T_1$, he should, for the case cited, obtain a value which is smaller than that obtained by the other observer. If he actually obtains the same value, there is only one explanation of the result. Either his scales or clocks, or both, must differ from those of the fixed observer. Now Einstein found how the scales and clocks of the moving observer would have to be related to those of the fixed observer in order that both should measure the same value for the velocity of light. Stated in mathematical terms, the relations between the measures of two observers who move relatively to each other with a velocity v is such that if each observer sets up a set of coördinate axes, with the velocity v parallel to the axis of x, and if the coördinates of a point as measured by the fixed observer are denoted by x, y, z, at the time t, the corresponding values which the moving observer will assign to the point are x', y', z' and t', where

$$x' = \beta (x - vt) \tag{1}$$
$$y' = y \tag{2}$$
$$z' = z \tag{3}$$
$$t' = \beta \left(t - \frac{v}{c^2} x \right) \tag{4}$$

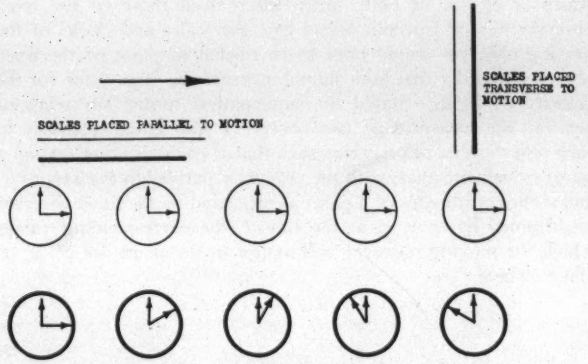
where we have written β for $1/\sqrt{1 - \frac{v^2}{c^2}}$, and c is the velocity of light.

No explanation of why there should be any difference in the measures is offered. The conclusion is simply inferred as a consequence of experimental facts which can all be correlated under the belief that both observers would measure the same value for the velocity of light.

Now equations (1)–(4) are about as innocent in appearance as we could wish and yet, in view of our firmly ingrained feeling about space and time, they lead to what appear at first sight as very astonishing conclusions. I shall not dissect these conclusions in detail, but shall content myself with stating some of them. These equations tell us that if the moving observer's scale stands with its length perpendicular to the direction of motion, the fixed

observer on measuring it will find it of the correct length. If, however, it stands parallel to the direction of motion, the fixed observer will find it too short (Fig. 3). Moreover, in terms of the clocks of the fixed observer, the clocks of the moving observer will all go too slow, and not only this, but they will be out of step with each other to extents which are greater and greater as we proceed along a line which is parallel to the motion. The shortening for a relative velocity equal to that of the earth in its orbit

FIG. 3.



only amounts to about 6 centimetres on the diameter of the earth, and the moving clocks only lose one second in two and a half years; but, for a velocity equal to that of light, the scales would become infinitely short and the clocks infinitely slow.

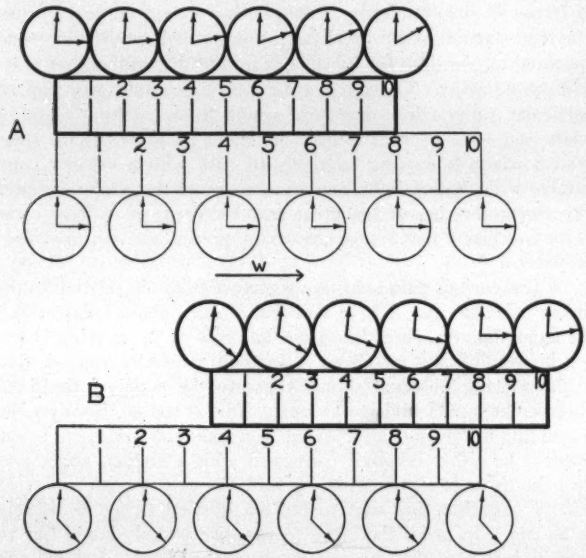
Of course, the moving observer would not realize the great shortening, for any scale which he used to test matters with would suffer the same contraction. The scale would not even appear short to him, because what he sees is determined by the image on the retina of his eye, and every little detail of structure of his retina would have suffered the same contraction, so that the images of the various parts of his scale would fall on the same parts of his retina as if everything were at rest. He would not appreciate the slowness of his clocks. For, any clock which he used to test matters with would go slow in just the same way. Even if, on account of the slowness of his clocks, he had to wait a thousand years between his breakfast and his lunch, he would not know it.

He would not feel any great hunger, because, in the ultimate nature of things hunger is determined by the progression of the digestive processes, and these would take place very slowly in corresponding proportion. As Professor Eddington has remarked, "Herein is to be found the secret of perpetual youth"; for, if you should cruise about in space with a velocity comparable with that of light, everything will go on with extreme slowness in terms of the standards of those who remain on terra firma. Having started after breakfast, you would return in what appeared to you time for lunch only to find that millions of years had passed away. Of course your long life would give you no particular satisfaction since you would know nothing about it while you were on that cruise. If there be some distant solar system which is moving relatively to ours with a velocity comparable with that of light, and an astronomer there who can peer into this room, he, or his great-grandchildren, must have come to the conclusion that I have already lectured to you for thousands of years.

A few curious paradoxes are sometimes cited in relation to the theory of relativity. I have said that if one, whom I have called the fixed observer, should measure the scale of the moving observer, he would find it too short. You might naturally conclude that if the moving observer should measure the scale of the fixed observer he would find it too long. This is not so, however, he would find it too short also. When you make this statement to the layman, he either becomes consumed with a sudden anger and says the thing is all nonsense, or he looks at you with a sort of fearful suspicion, and tries to make a hurried escape as politely as he can. In order therefore to reassure you, I should like to explain just how this strange result comes about. Let me ask you to place yourselves in the position of the fixed observer, and imagine that the scales and clocks drawn in thin lines in Fig. 4 belong to you and that it is your intention to measure the other scale which, together with myself, shall be supposed in motion in the direction of the arrow W. Referring to Fig. 4, A, what you do when you measure my scale is this: You read the left-hand end when your clock there points to a certain mark, and you find it zero. You read the right-hand end when your clock there stands at the same mark, and you find it 8, so you say the length of my scale is 8 minus zero = 8. However, I shall disagree with your

measurement entirely. For, in terms of my clocks, which are represented by thick circles, you have not read both ends of the scale at the same time. Now let me proceed to measure your scale in a manner which is to me correct. I shall read the left-hand end when the hand of my corresponding clock stands at the position shown at *A* in the figure and shall call it zero. In terms of

FIG. 4.



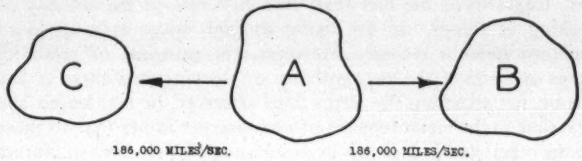
my clocks, and of my intuition also, it is not yet time to read the other end of the scale. You will think I am delaying my reading, but I do not think so, and finally, when I do take the reading of that end of your scale, with my clock hand standing at what I consider its proper position, the situation will be that represented in Fig. 4, *B*, and I find that the division 10 of your scale stands at my 8, so that, subtracting my two readings I shall find, for the length of your scale, the value 8 minus 0, and, to me, your scale is as much shorter than mine as you found mine shorter than yours.

The apparent paradox which exists as regards measurements

of length exists also as regards the measurement of time. The resting observer regards the moving observer's clocks as going too slow; but, when everything is considered properly, the moving observer regards the resting observer's clocks as going too slow also. In fact, it turns out there is a complete parallelism between the opinions of the two observers.

I may refer to one more apparent paradox. Suppose A (Fig. 5) is a body which we may think of as arbitrarily at rest, and that B is an airplane moving to the right with a velocity which an observer on A measures as 186,000 miles per second, the velocity of light. Suppose also that C is an airplane moving to

FIG. 5.



the left with a velocity which the observer on A measures as 186,000 miles per second. We might expect that if we could stand on C and measure the velocity of B, we should find twice 186,000 miles per second; but, this would not be so. We should find once 186,000 miles per second. What is the solution of this paradox? Well, when I spoke of the velocity of B being 186,000 miles per second, you will recall that I said "as measured by an observer on A." Now if we are on C, we shall entirely disagree with the measurement of the observer on A, for he is moving relatively to us with a very high velocity. We shall say that the body B is not moving relatively to him at all, and that the only reason he thinks it is, is that he has measured its velocity with a scale which is infinitely short, and clocks which run infinitely slow so that he has got a doubly infinitely wrong result.

Now it is possible to solve equations (1)–(4) for x, y, z, t in terms of x', y', z', t'. The result is readily seen to be

$$x = \beta (x' + vt') \tag{5}$$

$$y = y' \tag{6}$$

$$z = z' \tag{7}$$

$$t = \beta \left(t' + \frac{v}{c^2} x' \right) \tag{8}$$

so that, remembering that, if an observer B moves with a velocity v as measured by a fixed observer A, observer A will move with a velocity $-v$ as measured by B, we see that these equations which tell us how to find A's measures in terms of B are of exactly the same form as those which tell us how to obtain B's measures in terms of A. As far as this matter is concerned, there is absolutely nothing which can be said about the measures of A which cannot also be said about the measures of B.

We have arbitrarily spoken of the fixed and moving observers; and, owing to that unconscious tendency of every individual to think that he is right and the other man wrong, we have probably always thought of ourselves in the position of the resting observer, forgetful of the fact that, even in terms of the old way of looking at things, we are flying through space at a speed of eighteen miles a second. However, the principle of relativity takes upon itself the responsibility of denying that there is any reason for attaching the terms fixed observer, or real length and real time to the measurements of one observer rather than to those of the other, and it does this by postulating that the laws of Nature are such that if we should write them down in terms of the quantities which would be measured by one observer, and then, by means of equations (5)–(8), replace those quantities by their values in terms of those measured by the other observer, the form of the law would be unaltered.

Thus, to take an illustration, suppose we consider the well-known Newtonian law of gravitation as applied to the case of two bodies, a and b. If f_a, g_a, h_a are the component accelerations of the body a parallel to the axes of x, y, and z, respectively, if x_a, y_a, z_a are the coördinates of a, and x_b, y_b, z_b, those of b, and if the masses of a and b are respectively m_a and m_b, the motion of a is governed by the equations

$$m_a f_a = \frac{G m_a m_b (x_b - x_a)}{\left[(x_b - x_a)^2 + (y_b - y_a)^2 + (z_b - z_a)^2\right]^{\frac{3}{2}}} \tag{9}$$

$$m_a g_a = \frac{G m_a m_b (y_b - y_a)}{\left[(x_b - x_a)^2 + (y_b - y_a)^2 + (z_b - z_a)^2\right]^{\frac{3}{2}}} \tag{10}$$

$$m_a h_a = \frac{G m_a m_b (z_b - z_a)}{\left[(x_b - x_a)^2 + (y_b - y_a)^2 + (z_b - z_a)^2\right]^{\frac{3}{2}}} \tag{11}$$

where G is the well-known gravitational constant. Now by means of equations (5)–(8), we can express the quantities x_a, x_b, y_a, y_b, z_a, z_b in terms of the primed quantities and substitute the results on the right-hand side of equations (9)–(11). Moreover, with a slightly greater difficulty we can spell out what the accelerations f_a, g_a, h_a would be if expressed in terms of accelerations, velocities and so forth in the primed system of coördinates, and we can substitute in terms of these primed quantities on the left-hand side. If it should so happen that when all the necessary cancelling is done and rearrangement of terms is made, the equations are found equivalent to [1]

$$m_a f_a' = \frac{Gm_a m_b (x_b' - x_a')}{\left[(x_b' - x_a')^2 + (y_b' - y_a')^2 + (z_b' - z_a')^2\right]^{\frac{3}{2}}} \tag{12}$$

$$m_a g_a' = \frac{Gm_a m_b (y_b' - y_a')}{\left[(x_b' - x_a')^2 + (y_b' - y_a')^2 + (z_b' - z_a')^2\right]^{\frac{3}{2}}} \tag{13}$$

$$m_a h_a' = \frac{Gm_a m_b (z_b' - z_a')}{\left[(x_b' - x_a')^2 + (y_b' - y_a')^2 + (z_b' - z_a')^2\right]^{\frac{3}{2}}} \tag{14}$$

we should be able to say that this Newtonian law was of the same form whether expressed in terms of the measurements of A or in terms of the measurements of B. The actual accelerations as measured by A and B would be different, and the distances would be different, but the relation between what A measured as the accelerations and what A measured as the distances would be the same as the relation between what B measured as the accelerations and what B measured as the distances.

Now as a matter of fact, this condition would *not* prevail for the Newtonian law which we have written down, and so the principle of relativity would deny that this law represents an exact statement of the truth. This does not mean that it is not possible to find a law which may be very near to the Newtonian law and may yet satisfy the conditions which relativity imposes on it. As a matter of fact, it *is* possible to find such laws, indeed an infinite

[1] It is hardly necessary to remark that the test would not require that (9), (10) and (11) go over into (12), (13) and (14) in such a way that (9) goes into (12), (10) into (13), and (11) into (14); but that the equations into which (9), (10) and (11) go over *contain* (12), (13) and (14) as a consequence.

number of them. I shall not write any of them down, as they are a little complicated in form, and it is not my intention to discuss them in detail. Relativity does not claim to find the law for us. It cannot do that. Its function is to act as a sort of supreme court which will pass upon the laws we propose, so that, if we have faith in it, we shall at least be guided in our search. Herein lies the great practical value of the principle; for, in spite of the limitation of its functions to a sort of judiciary character, the conditions which it imposes are sufficiently severe to limit our field of search very considerably and sometimes, when taken in conjunction with other considerations, they limit the choice to only one possibility.

But you may say, "What is the justification for such great faith in the principle?" The answer is that our experiments having to do with the velocity of light have forced us to the conclusion that the measures of observers who move relatively to each other are related in the way we have stated in equations (1)–(4); and, with this granted, if the laws were not such as to be unchanged on transforming from one set of measures to another, we should expect to be able to devise experiments which would give us different results at different times of the year, on account of the change in the earth's motion; or, what amounts to very much the same thing, we should be able to obtain evidence of our state of motion by performing experiments in which the apparatus used was orientated in varying directions with regard to the supposed earth's motion. Many such experiments have been tried and all have failed to reveal any difference as a result of the change in the earth's motion or in the orientation of the apparatus.

If we accept the principle, there is a complete reciprocity between the measures of two observers who move relatively to each other with constant speed, and there is absolutely nothing which one observer A can do to prove that his measures of distance and time are correct while B's are wrong that B cannot do to prove that his measures are correct and A's wrong. With a condition of affairs such as this the whole meaning of absolute motion, absolute distance and absolute time vanishes. The measures of distance and time still play their part for the individual observers. All that they have lost is this adornment of absoluteness which

never did anything for anybody. The gold braid of the hotel porter has been found unessential for the performance of his duties.

But, I think I hear someone say, "Surely this must be nonsense; for, what is this infidel talking about when he says there is no absolute length and no absolute time. I know in my bones that these things exist. I may not be able to say exactly what I mean by them, but I know they are there." I have met many people who have talked this way. I sometimes think they feel that they have an intuition about this matter which is denied to other mortals, and particularly to the man they are talking to. They rather pity him for not feeling this conviction of absoluteness. They regard him as a musician regards one who sees nothing in the music of Beethoven. If there be any such in the audience this evening, will they believe me when I assure them that the relativist has had those very feelings of which you think he was never conscious. He knows them very well indeed. He has dwelt with them and worked with them. He has fought for many years to find some way in which they might be made to live, and only when he found that their home was not in our universe at any rate did he reluctantly let them go.

I have referred to the fact that the principle of relativity acts as a guide in our search for the laws of Nature by requiring that the laws shall be of such a form as will remain unchanged when a transformation is made from the measures of one observer to those of another who moves relatively to him with constant speed. At first sight it may appear an insuperably difficult matter to construct laws which shall conform to this requirement. Happily, the mathematician has taught us how to handle this problem. Time will not permit me to elaborate on details of technique; but, the underlying principle is so powerful that I cannot forego the temptation to say a word about it even though, in doing so, I must of necessity omit much that relates to the most beautiful aspects of the subject, those aspects which have to do with the moulding of space and time into a single framework, and which we owe to the genius of the Polish mathematician, Minkowski.

To illustrate the idea to be brought out, suppose we consider the well-known equations which connect the electric current through an area with the work done in carrying a unit magnetic

pole around it. These equations when expressed in differential form are, as is well known,

$$4\pi i_x = \frac{\partial H_z}{\partial y} - \frac{\partial H_y}{\partial z} \tag{15}$$

$$4\pi i_y = \frac{\partial H_x}{\partial z} - \frac{\partial H_z}{\partial x} \tag{16}$$

$$4\pi i_z = \frac{\partial H_y}{\partial x} - \frac{\partial H_x}{\partial y} \tag{17}$$

where i refers to current density, and H to magnetic field.

Now suppose we should take a set of axes, which I shall denote by x', y', z', which are inclined to the set to which (15)–(17) apply. It would of course be possible to resolve the currents i_x, i_y, i_z along these new axes, and find the components i_x', i_y', i_z', along them. It is also possible to express the components H_x, H_y, H_z, of the magnetic field along the old axes in terms of the components H_x', H_y', H_z', along the new axes, and the differential coefficients along the old axes in terms of those along the new. When all of this is done we should feel greatly surprised if the new equations were not found to be the equivalent of

$$4\pi i_x' = \frac{\partial H_z'}{\partial y'} - \frac{\partial H_y'}{\partial z'} \tag{18}$$

$$4\pi i_y' = \frac{\partial H_x'}{\partial z'} - \frac{\partial H_z'}{\partial x'} \tag{19}$$

$$4\pi i_z' = \frac{\partial H_y'}{\partial x'} - \frac{\partial H_x'}{\partial y'} \tag{20}$$

although if any one unfamiliar with the type of algebra involved were to try to prove the matter he would probably find the proof by no means easy.

As a matter of fact, if in place of a minus sign between the terms on the right-hand side of our equations (15)–(17) there had been a plus, they would not have reverted to the same form on changing the axes. If such a situation actually prevailed in Nature, it would mean that the laws of Nature would be different for different directions in space. It would be as though the whole universe were built in a crystal whose properties were different in different directions.

When we expect that the laws which we write down shall be of the same form whatever the axes we may use in terms of which to express them, we are really implying a belief which,

natural as it seems to us, is something which might not have corresponded to facts. The reason that the equations (15)–(17) do have the property which we desire is that the quantities on the left-hand side constitute the components of a vector, and the quantities on the right-hand side constitute the components of a vector. Let me say more clearly what I mean by this. If we measure x, y, z, the coördinates of a point in space with reference to a certain set of axes, we may express in terms of them, the coördinates x', y', z' of that point with respect to another set of axes. The law is very simple. It is of the form

$$\begin{aligned} x' &= l_1x + m_1y + n_1z \\ y' &= l_2x + m_2y + n_2z \\ z' &= l_3x + m_3y + n_3z \end{aligned}$$

where the l's, m's, and n's are constants depending on the relation of the new axes to the old. This is the law by which the old axes "transform" into the new axes. Now suppose there be another set of quantities X, Y, Z, which I shall suppose defined at a point in some way in relation to these axes, for example, as the respective component accelerations which a particle experiences when placed at that point, or as proportional to the product of these accelerations with the corresponding velocities. Suppose I now make the same definitions for quantities X', Y', Z', but in terms of the corresponding quantities, accelerations, velocities, etc., in the x', y', z' system. It will of course be possible to find how the quantities X', Y', Z' are related to X, Y, Z. The result will depend upon what I have defined X, Y, Z to be. If, as a result of my definition it turns out that

$$X = l_1X' + m_1Y' + n_1Z' \tag{21}$$
$$Y = l_2X' + m_2Y' + n_2Z' \tag{22}$$
$$Z = l_3X' + m_3Y' + n_3Z' \tag{23}$$

the quantities X, Y, Z may be called the components of a vector. This is the property which is of significance in a vector. To say that a vector is a directed quantity means very little without amplification. Thus, an angular velocity is a vector and its direction is perpendicular to the plane of the rotation, but that is really the one direction in which nothing is happening. The significance of the statement that an angular velocity is a vector and that the direction of the vector is perpendicular to the plane of rotation, lies in the fact that if the angular velocity be multiplied succes-

sively by the cosines of the angles which the perpendicular in question makes with the three coördinate axes, the three quantities obtained constitute the components of a vector in the sense that in transforming to other axes they transform in the manner indicated by equations (21)–(23).

Now it is very easy to see that if we equate two vectors, or if we form three equations by equating their components, the relations obtained will be of the same form when transformed to other axes. Both vectors transform in the same way. We have become so accustomed to vectors and scalars in physics that we know them, as it were, unconsciously, and it never occurs to us to equate sets of three quantities X, Y, Z which do not form the components of a vector. Even were we less familiar with vectors so that we could not recognize them at sight, however, we know of mathematical tests which would enable us to tell whether those quantities did or did not constitute the components of a vector, and we know of methods by which we can form vectors, *i.e.*, methods by which we can write down quantities which are vectors.

And so, we have learned how to write down quantities which are called 4-vectors. A 4-vector is a set of four quantities X, Y, Z, T, which are defined in such a way in relation to quantities depending on space and time that if we make corresponding definitions in the two sets of measures denoted by x, y, z, t and x', y', z', t', and related by equations (1)–(4), the quantities X', Y', Z', T' are related to the quantities X, Y, Z, T, by the equations [2]

$$X' = \beta(X - vT) \tag{24}$$

$$Y' = Y \tag{25}$$

$$Z' = Z \tag{26}$$

$$T' = \beta\left(T - \frac{v}{c^2}X\right) \tag{27}$$

which are of the same form as (1)–(4). It can readily be seen that provided the equations which we write down as expressions of the laws of Nature constitute relations between 4-vectors, they

[2] Those familiar with the subject will recall that, in the conventional notation, the components of the 4-vector are taken as X, Y, Z, U, where $U = icT$, and $i = \sqrt{-1}$. It will also be recalled that Minkowski introduced the idea of a 6-vector, which is a set of six quantities which transform as an element of area in four dimensions. It is not practicable to introduce here any discussion of the 6-vector and is unnecessary for the purpose in hand, which is to illustrate the fundamental idea involved in writing down invariant relations.

may be wrong, but they will at least conform to the requirement which relativity imposes, that they shall be of the same form whether expressed in terms of the measures of one observer or of another who moves relatively to him with constant rectilinear velocity.

Minkowski illuminated his theory by drawing a very close parallel between the transformation which corresponds to a rotation of axes in three-dimensional space and the transformation represented by equations (1)–(4). Indeed, he showed that equations (1)–(4) can be interpreted as a rotation of axes in what the mathematician would call a four-dimensional space, in which to our three dimensions of ordinary space we have added a fourth corresponding to the time. These geometrical ideas are very illuminating to those who have thought a great deal about the subject; but, as they only torment those who are not specialists in the field, and as they are really unnecessary (though very useful) for stating what the principle of relativity is, I do not propose to say anything about them here.

The theory of relativity of which I have been speaking was put forward by Einstein in 1905. In 1915, however, Einstein brought out a much more generalized theory, a theory much more radical in respect to the standpoint which it takes in relation to what may be considered as absolute. This theory caused such a sensation in the scientific world that the noise thereof reached out into the popular world, and the layman heard for the first time of the older theory of relativity. Since the older theory of relativity has in it some of those more appealing paradoxes to which I have referred, the layman found plenty to occupy his attention in thinking about them. The result is that popular attention, while thinking that it was concerning itself with the general theory of relativity of 1915, was really occupying itself for the most part with the older theory of which I have spoken. I must not give the impression that the older theory has been discarded. It still continues to perform its function within the wide field of its application, and what the general theory has done has been to extend the ideas whose germ was found in the restricted theory to a much wider generality and in a much more philosophically satisfying manner.

To illustrate the matters of which I now wish to speak, let me ask you to consider that I have a scaffolding extending up into

the sky, with three sets of rods, mutually perpendicular to each other, and suppose that there are on these rods marks which are equally spaced, in the sense which you understand that phrase, and are labelled 1, 2, 3, 4, etc. Suppose further that I am provided with a clock on whose face there are also marks which again are numbered 1, 2, 3, etc., giving me what I would call equal intervals of time. Suppose a sun and a planet move about in this scaffolding, and suppose I consider the Newtonian law as expressed by equations (9)–(11), as a candidate for the expression of the facts of the case. What this means is, of course, perfectly obvious. It means that I take a very large number of readings of the position of the planet a and the sun b, for example, and the corresponding times, so that I am able to express the rate at which the position of the planet changes (its velocity), and the rate at which the velocity changes (the acceleration), and that I proceed to test whether the component accelerations are related to the corresponding space measures by this formula. I am not particularly concerned with discussing what correction I make on the time to allow for the time taken for the light from the planets to reach me. I simply suppose that I have decided upon some way of reading my clock, and possibly correcting its reading, for the purpose of obtaining a number t_a which I can associate with each value of x_a, y_a, z_a, which I measure for a, and a number t_b which I can associate with each value of x_b, y_b, z_b; and, I suppose that having written my data down on paper I proceed to test the law represented by equations (9)–(11). Suppose that I find that the law holds. Then that may well be the end of the matter. But, it will be a most astonishing thing if this does happen *exactly* for the reason which I shall try to make clear.

Suppose that I make each rod of my framework of scaffolding double, and that, leaving one frame which I shall call A as it is, I solder the points of intersection of the other, which I shall call B, and then distort the frame in an arbitrary way, leaving the same numbers attached to the points of intersection of the rods. Suppose further that I do a similar thing as regards the clock. In other words, suppose that I duplicate my original clock and leaving the old one as belonging to the system A, I modify the other one, which shall belong to B, in such a manner that it goes highly irregularly, and suppose that I modify my way of using its readings to assign times to the instants when

objects coincide with certain intersections on the frames. Suppose I do this in a perfectly arbitrary manner so that all that is left of the connection between the new time readings and the old is the fact that for every time reading that I assigned with my A clock as corresponding to the coincidence of a body with a point of intersection of my A frame, there corresponds one, and only one, time as recorded by my B clock for that same coincidence. I shall even go farther and imagine, that as time (recorded by the A clock if you wish) increases, the twisting of the B frame and the modification of the A clock alter. Moreover I shall suppose that the B clock and the method of calculation used in conjunction with it are continually changing, so that even the *relation* between the time readings recorded by the B and A clocks for coincidences of bodies with a given point of either frame changes with the time. Suppose I now use the numbers on the B frame to test the law given by (9)–(11), which we supposed correct when the A system was used. It is hardly to be expected that I shall find the law to hold now. But you will say "of course not, the numbers are all wrong." Let us inquire a little further into this accusation against the numbers, however.

Suppose that when I asked you to imagine that the framework B became distorted I had asked you yourselves to take part in that distortion in every detail. Suppose, in fact, everything took part in this distortion. Then you would be totally unconscious of the change. For, every intersection of two or more lines would be preserved in that distortion in the sense that, if there were a certain identifiable molecule at the point of intersection in the old frame, that same molecule would be found at the corresponding point of intersection in the distorted frame. There might be a scale on which the numbers 1 and 2 and 2 and 3 were not separated by anything like equal distances as I viewed them, I who had not taken part in the distortion. As you viewed them, however, they would appear quite satisfactory; for your retina would be distorted, too, and the images of the numbers 1, 2, and 3 would fall on the same retinal cells as they fell on before the distortion, so that you will be quite satisfied with the picture. It is true that the A rods would appear crooked to you and the numbering on them would appear wrong. You might think that you could discover the inequality of distances between the marks on the B rods referred to above by using a movable scale which just fitted

between the marks 1 and 2, and which you might suppose would fail to fit the space between 2 and 3. This supposition would be wrong, however, for, in terms of my standards of measurement, the rod would change in length as you moved it, and in such a way as to preserve the illusion. You would appear to me curiously deformed; but you could not test the crookedness of your backs because the straight edges you used would be crooked, too. You would not even look crooked to each other, because the distortion of the rays of light, and of the retinas of your eyes, would be such as to bring the images of the various parts of your neighbors on to the same retinal cells as they fell on before the transformation. You might change your form as you moved about, but you would not know it for similar reasons. The same sort of thing would happen as regards the time. You could not discover the irregularity in your clock because any other clock you used to test it would suffer a like irregularity. You could not time it by your pulse beat because that would go irregularly, too. In fact, to you, everything associated with that crooked measuring system would appear correct. The most humiliating thing from my standpoint is that to you I should appear deformed, and possessed of a highly irregular pulse. I should appear to alter my dimensions as I moved about; and, in fact, there would be absolutely nothing which I could do to prove that my system was undeformed and yours was deformed, that you could not do to prove that yours was undeformed and mine deformed. This being so, how do you know what the situation may be at this moment? If I miss out the introduction to this discussion, the picture of the deformation of the framework, etc., and simply start the story where we find ourselves with the difference of opinion as to which scheme of measures is right and which is wrong, you will readily see that there is no way of settling the question. In fact, there is no question to be settled. There is no meaning to one being right and the other wrong. If you think there is, state it, and I will show how the advocate of either system will be able to prove in terms of your criterion that his is the right system. In fact, both systems are on a par with each other; and, since, as you will recall, the difference which I supposed to exist between the labelling of the two systems and the differences between the clocks was perfectly arbitrary, for it arose from an arbitrary distortion of the framework and arbitrary changes in the

true measurements, all systems must be regarded as on an equal footing. Now if you can only eliminate from your consciousness the idea that one of these systems is right and all the others wrong, and you certainly ought to be willing to do this unless you can say what you mean by one being right, you will be in a position to admit that if we write down any law of such a kind that it is true when one measuring system (one method of attaching numbers to the events) is adopted and is untrue when another system is adopted, that law must have in it some properties of this system of measurement we have chosen. You will reply, "Of course it has. That is the object of physics, to choose a measuring system and then find laws expressed in terms of it." But if, as I have tried to emphasize to be the case, the system of measurement which we have chosen is a perfectly arbitrary one, should we expect to be able to express in terms of it, and in a simple form, any intrinsic regularity which there may be in Nature? Of course, we certainly can write down laws which are very approximately correct under certain conditions; but have we any right to expect simplicity in the particular set of measures we have if the laws, when transformed to any other set of measures which are on equal footing with ours, assume another form and in general a form which is almost infinitely complex? How much happier we should feel if that which our laws expressed were the same story in all systems of measures.

I may draw a crude parallel by imagining a great poet who conceives the idea of a beautiful poem which he symbolizes in his actions. And then comes one who writes down in the English language what he considers to be in the mind of the poet. Then he translates it literally into French, and finds that it does not express the same idea. Of course, it may be impossible to express the ideas of this poet in any language. But, if we go on trying until we have written a poem which means the same thing to us when it is translated into French, German, or any other language, we shall feel that we have so far eliminated the individual peculiarities of the languages as to have a better chance of our poem expressing the ideas of the poet than if it had meant a different thing in each language.

And so the idea of the relativist is to express the laws of Nature in such a form that, if we translate them into other systems of measures, they say exactly the same thing. In the

technical language of the mathematician the laws must be of such a form that they are invariant under any transformation of coördinates, the coördinates referring to the three measures of length and one of time.

The matter may be put mathematically thus: Suppose x, y, z, t refer to the numbers which we assign to the three measurements of space and the one measure of time corresponding to a certain point. Suppose that I alter the labelling attributed to my scales and the times recorded by my clocks so that the new value of x which I shall assign to that point is different from what it was before. Suppose it is

$$x_1 = F_1(x, y, z, t)$$

where I may say, for the benefit of those who are not mathematicians, that all I mean by this is that I have specified some way in which you are to calculate the new coördinate x_1 for the point in terms of the old coördinates x, y, z, and t. Suppose similarly that y_1 is given by F_2 (x, y, z, t) and so on, so that

$$x_1 = F_1(x, y, z, t) \quad (28)$$
$$y_1 = F_2(x, y, z, t) \quad (29)$$
$$z_1 = F_3(x, y, z, t) \quad (30)$$
$$t_1 = F_4(x, y, z, t) \quad (31)$$

Suppose that the law expressed by equations (9)–(11) had been invariant under this arbitrary transformation as it is called. Then, it would have happened that if, having first written the law down in terms of x_1, y_1, z_1, t_1, I had everywhere substituted the above expressions for x_1, y_1, z_1, t_1 in terms of x, y, z, and t, and if for the acceleration in terms of the coördinates with subscripts unity I had substituted their values in terms of the coördinates with no subscripts, the F's would mysteriously have disappeared, and the equations would have reverted to the same form in the x, y, z, t coördinates as they formerly had in the x_1, y_1, z_1, t_1 coördinates. Of course, I need hardly say that this happy consummation would not materialize in this case; and if you should try to modify the law so that it would materialize, you would probably get into such hopeless confusion that you would come to the conclusion that nobody could write down any equations, right or wrong, which could obey such a drastic requirement. The poem of the poet would be so remarkable that it could not be written in any language. Hence it could not be written in all.

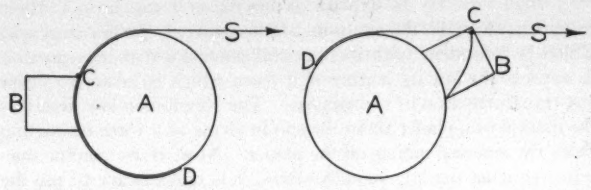
In spite of this, it turns out that it is possible to write down equations which obey this very drastic requirement. I have already spoken of vectors as being quantities which when equated to each other give equations which remain true when the particular transformation corresponding to rotation of axes is made. Quantities which satisfy the corresponding requirement for any arbitrary transformation of coördinates are called tensors. They were studied by the pure mathematicians, Levi-Civita and Christoffel, without any thought that they could ever be of service to the physicist; and, one of the great strokes of Einstein's genius consisted in his seeing in the material of these mathematicians the very bricks wherewith to build his great theory of general relativity.

I shall now try to explain as briefly as I can how Einstein sought to describe the motions of the heavenly bodies in a way which is consistent with this general postulate that it is possible to express the law of Nature in a form which is invariant under any transformation of coördinates. The Newtonian law describes the motion of a planet about the sun in terms of a force emanating from the sun and acting on the planet. Now, if we confine ourselves to what the law actually states, it is unnecessary to use the word " force " at all. The law is summed up (at least for the case of two bodies) in equations (9)–(11). These state how it is possible to calculate the acceleration of the body A in terms of its distance from the body B, and that is the end of the matter. The equation would tell the story of the motion of the planet if nobody had ever used the word " force." However, we customarily describe the equation by saying that the sun produces a force proportional to the product of the two masses concerned and inversely proportional to the square of the distance between them, and that this force accelerates the planet in the line of its action and to an extent proportional to its intensity. If the planet were very far away from the sun it would move in approximately a straight line with a constant velocity; and under that condition we would say there is no force acting on it. As a matter of fact this is what we mean by there being no force on the body. Absence of everything but uniform velocity represents the condition of nothingness which the mind takes as its origin from which to elaborate its thoughts. If the body is moving in a straight line with a constant velocity the mind is content, no questions are asked. If, however, it is

moving otherwise, we feel it incumbent upon us to inquire why. We inquire particularly as to the acceleration of the body because that is the quantity which, in a large number of cases, can be simply expressed in terms of the positions of the other bodies. It is for this reason that we *define* the direction of the force as the direction of the acceleration in elementary mechanics, and the magnitude of the force as proportional to the magnitude of the acceleration.

But let me ask you to take part with me, at any rate, in imagination, in an experiment. Let me place myself on a rotating disc out in space, while you remain where you are to watch the performance. For simplicity, let me suppose that gravity

FIG. 6.



is eliminated. Now, if I am in the condition of mind of the people of the reign of King Henry VIII, I shall believe that the disc does not rotate, and shall seek to hang anybody who tries to make me believe otherwise. If I let go a stone, you of course know that that stone will continue to travel along the tangent at its point of release, but with uniform velocity. To me, however, the stone will appear to describe a complicated curve in space.[3]

If I have been brought up in the dogma that a body moves in a straight line unless a force acts upon it, I shall say that the

[3] Here was shown an experiment in which a circular disc *A* (Fig. 6, left-hand figure) was attached to a disc of glass, a portion of which, *B*, was blackened with smoke to represent a blackboard. The knob *C* was attached to a string which was curved around a pulley immediately in front of the disc and fastened to a point *D*, so that by pulling the string the disc could be made to rotate. The conditions were arranged so that the string was pulled horizontally and the knob departed from the circle, and so from the corner of *B* at the instant when it reached its highest point. The knob was adjusted so as to press against *B* and so rub the smoke off it. A curve such as that shown at the right of the figure was obtained, and this represented how the flight of the stone appeared to the observer on the disc.

disc repels the stone, and I shall try to find the law governing the variation of the force with distance. I may go farther, and try to imagine some reason for this force, some pushing action transmitted from the disc to the stone through the surrounding medium; and, you will pity me for all this waste labor, particularly for my attempt to find a mechanism for the force when you know that, if I would only accept measurements from your standpoint, all would be so simple. To me, however, this force will be a very real thing. I shall feel it. My head will try to leave my body and fly off into space, and I shall have to be tied down to the disc or I shall fly away altogether.

The change of viewpoint between me and you in the foregoing illustration is simply a special case of transformation of coördinates. How nice it would be if we could sum up all the motions of things in our universe in the simple statement that all bodies would be found to be moving in straight lines with constant velocities if we would only adopt the proper system of coördinates in terms of which to make our measurements. All ideas of gravitational forces could be dismissed from our minds, and replaced by this simple, fundamental starting point. We should not be changing the laws of Nature but only our way of describing them.

Unfortunately, things do not go as simply as this; but, suppose they did, how should we describe the matter in mathematical language? We should first imagine ourselves in this favored system of coördinates where everything moved in straight lines with a constant velocity. Now the mathematician has a very concise way of presenting the two-fold idea straight line, constant velocity. To say how he does it, let me remind you of two ways in which we could describe a straight line. We could say that a straight line is such a path that if you represent it in rectangular coördinates, the change of y is proportional to the change of x and the change of z is proportional to the change of x. You could, however, say the same thing much more concisely in the statement that the straight line which joins two points is the shortest distance between those two points.

You will recall that, according to the Pythagorian theorem of geometry, the distance s between two points A and B is given by the sum of the squares of the coördinate differences for those two points,

$$s^2 = (x_a - x_b)^2 + (y_a - y_b)^2 + (z_a - z_b)^2.$$

If the two points are very near together, the mathematician writes ds for the distance between them, and dx, dy, and dz for the difference in their coördinates, so that

$$ds^2 = dx^2 + dy^2 + dz^2 \tag{32}$$

and

$$ds = \sqrt{dx^2 + dy^2 + dz^2} \tag{33}$$

Suppose then we ask ourselves the question, what sort of path must join two points A and B in order that if we add up the values of ds along that path, *i.e.*, if we take the sum of the $\sqrt{dx^2 + dy^2 + dz^2}$ along the path, the result shall be less than for any other path? You know the result at once, and the mathematician can demonstrate it algebraically even though you did not know it. The path is a straight line in the sense that along that path changes of y and of z will be proportional to changes of x.

Now suppose I should consider a particle which occupies two different points A and B, at two different times, and should inquire what path a particle should take between them and how its speed should vary along that path in order that $\sqrt{dx^2 + dy^2 + dz^2 + dt^2}$, which I shall call ds, shall be less than for any other path and state of motion along that path which takes the particle from A to B in the given time. This ds does not have quite as much meaning to you as the old ds, although you must admit some resemblance in its appearance. I must not stop and yield to the temptation to which the mathematician succumbs, to try to consider the new ds as the length of a line in a four-dimensional space, but will content myself with the statement that the answer to the problems of how x, y, and z must vary with t in order that the sum of the ds's along the path shall be a minimum is a perfectly definite one, and the problem is a perfectly easy one for the mathematician. The answer is that x, y, and z shall all be proportional to t. But this is the same thing as saying that the particle must move in a straight line with constant velocity. Thus, we could sum up the statement that bodies move in straight lines with constant velocity by saying that they move in such a way that if between any two points A and B we add up all the ds's where

$$ds = \sqrt{dx^2 + dy^2 + dz^2 + dt^2} \tag{34}$$

the result will be less than for any other path or mode of motion along the path.

Suppose that I should now consider how things would look in another system of coördinates x_1, y_1, z_1, t_1, which are related to the first in an arbitrary manner designated by equations

$$x = \varphi(x_1, y_1, z_1, t_1) \tag{35}$$
$$y = \varphi(x_1, y_1, z_1, t_1) \tag{36}$$
$$z = \varphi(x_1, y_1, z_1, t_1) \tag{37}$$
$$t = \varphi(x_1, y_1, z_1, t_1) \tag{38}$$

If I should express the dx, dy, dz and dt of (34) in terms of x_1, y_1, z_1, t_1, it is easy to show that ds^2 becomes

$$ds^2 = g_{11}dx_1^2 + g_{22}dy_1^2 + g_{33}dz_1^2 + g_{44}dt^2 + g_{12}dx_1dy_1 + g_{13}dx_1dz_1 + \cdots\cdots \tag{39}$$

an expression containing the products of every pair of the quantities dx_1, dy_1, dz_1 and dt_1. There are ten terms in it, and the g's could be specified directly you specified the φ's which determine how the new set of measures is to be related to the old. These g's would of course be functions of x_1, y_1, z_1, t_1. Now, in terms of the new coördinates x_1, y_1, z_1, t_1, the old path which was a straight line with constant velocity will look anything but a straight line with constant velocity. If, however, somebody would give me the right-hand side of (39) with the g's properly expressed as functions of the coördinates, and if I should take the path of the particle as it appeared to me, evaluate ds by (39), and add up all the ds's for the whole path of the particle, you know that I should find a value which would be less than I should find for any other path or mode of description of path which the particle could follow; for, the ds of (39) is numerically equal to that of (34).

Now you might think at first sight that with a sufficiently arbitrary transformation of coördinates as determined by (35)–(38), it would be possible to produce any sort of g's you liked to assign; and you might even go so far as to think that without further statement the fact that the sum of the ds's taken along the path should be a minimum would include every class of motion which could take place. This is not so, however. There is something in common for all the g's which can have come by even the most arbitrary transformation from the g's, 1, 1, 1, 1, of equation (34). That something is expressed by a series of equations

which the g's must satisfy. There are 256 of them, but happily a lot of them are the same, so that they boil down to 20. These equations, which, to use the techncial mode of description, are differential equations, are statements of how the changes of the various g's with changes of the coördinates are bound up with each other; and the interesting thing about them is that in them there is no statement of which set of coördinates is used. They are the same for all coördinates. In other words, if you should take your stand in any one system of coördinates, you could ascertain by the way in which the changes of the various g's with changes in the coördinates were related to each other, whether they were g's which could have come from the g's 1, 1, 1, 1, of equation (34); and, you could do this without knowledge of what the system of coördinates you were using might be. Suppose then I make the statement that, taken between any two points which the particle occupies at any two times, the path and mode of description of the path is such as to make the sum of all the quantities

$$\sqrt{g_{11}dx_1^2 + g_{12}dx_1dy_1 + \cdots\cdots}$$

a minimum, where all that is said about the g's is that they satisfy the equations of which I have spoken, then the statement which I make may be wrong; but in all systems of coördinates it describes the same path, and it is the same statement in all systems of coördinates so that it conforms to the requirements of the general theory of relativity.

Now the law is not quite as simple as this. However, we are tempted to see whether we may not describe the law in the above manner under the stipulation that these g's do not satisfy the condition to which I have referred, but some other equation which is the same in all systems of coördinates, so that the g's are of such a kind that no transformation whatever would reduce them to the g's 1, 1, 1, 1 of equation (34). Now what Einstein did was to find a set of equations which the g's must satisfy in order that he could describe the motions of bodies in terms of these in the manner I have sketched and yet in such a way that those motions corresponded very nearly to the motions as described by the Newtonian law. He found himself limited practically to one possibility, so that since we know that the Newtonian law is very nearly correct, we are tempted to believe that this generalized law is the right one. The Newtonian law predicted

that a planet would travel around the sun in a true ellipse, if we neglect the perturbation caused by the other planets; but, Einstein's law makes the major axis of that ellipse rotate very slowly, a result which is found to hold, particularly in the planet Mercury. Moreover, this is not all. For the departure from the Newtonian law is such that the velocity of a particle can never exceed a certain maximum value. This corresponds to what we find in nature, even when we do not limit ourselves to motions of the gravitational type. For, the highest particle velocities we find are to be found in the so-called β-rays which are emitted from radium, and none of them exceeds a limiting velocity which is, as a matter of fact, the velocity of light.

Further, the conclusion that this maximum velocity is equal to the velocity of light is in perfect harmony with the value which would be calculated for it from the observations on the departure of the orbit of Mercury from the strict Newtonian law.[4] The calculated value for the rate of rotation of the major axis of Mercury amounts to forty-three seconds of arc per century—on the basis of the assumption that the velocity of light represents the limiting velocity, and this value agrees with the experimentally measured value within limits of experimental error, which amounts to about four seconds of arc per century.

Again, since, on the above view, the velocity of light represents the highest velocity which a body can attain on the basis of the theory of relativity, it is natural to suppose that the paths of rays of light may be the same as those of particles which would travel

[4] Those who are familiar with the subject will observe that I have here modified the customary method of presentation by introducing the quantity c, in the first instance not as the velocity of light, but as the limiting velocity attainable by a particle. It then follows as an algebraic consequence that, far removed from matter the line element can be thrown into the forms corresponding to $ds^2 = dx^2 + dy^2 + dz^2 - c^2dt^2$. That this quantity ds is invariant under the Lorentzian transformation is now an algebraic fact, but this does not necessarily imply that the measures of observers who move relatively to each other with constant velocity far removed from matter will be related to each other by that transformation. The general theory does not contain the special theory in the full sense of the statement. To the extent that its form is conveniently adapted to the inclusion of the special theory by additional hypothesis, however, the general theory provides for the situation, not by special adjustment with that end in view, but as a result of the form of the line element which is necessary to make it lead to the Newtonian law for gravitational particle motion as an approximation.

with the speed of light. If we assume this, we can calculate how much a light ray would be deflected in passing by the sun. The amount comes out very small—only 1.75 seconds of arc for a ray which grazes the sun's surface; but, as we all know, this deflection has been verified on two occasions, by observations taken during a total eclipse of the sun. The plan was to photograph certain stars which appeared near the sun's disc (this being only possible during the eclipse) and their apparent positions could then be compared with the apparent positions when photographed at another time when they did not lie near to the sun's disc. In 1919, two British expeditions found 1.98 seconds and 1.61 seconds, respectively, as compared with the theoretical value 1.75 seconds, a fairly good agreement when the difficulties of the measurements are realized. During the eclipse of 1922, however, Professor Campbell made very careful observations at Wallal, Australia, with the result that the mean deviation found experimentally agreed with the theoretical value to an accuracy of 0.01 second of arc, an accuracy well within the limit of experimental error.

So far we have discussed what the theory of relativity has to say regarding the motions of planets and of light; but another important aspect concerns its bearing on phenomena in general. The condition of affairs near the sun is different from what it is far away. So far we have symbolized this by saying that the quantities which we have referred to as the g's are different. Now Einstein raises the question of how this difference will affect phenomena other than planetary motion—the period of a vibrating atom for example. I have already remarked that certain aspects of a gravitational field may be simulated by subjecting ourselves to an acceleration, as was the case with the rotating disc referred to above. Perhaps a simpler illustration is to be found in the case of an elevator which is descending with an acceleration. If you should hold up a weight, the pull on your muscles would be reduced by the downward acceleration of the elevator; and, if the elevator be allowed to *drop* freely, so that its acceleration is that which gravity would impart to it, you would find no pull at all. In fact, if you released the weight, it would stand by itself in free space. Of course, it would really be accelerated downwards along with the elevator; but, in terms of a reference system fixed in the elevator it remains at rest, sus-

pended on nothingness like Mohammed's coffin. The transformation of your reference system from terra firma to the moving elevator would apparently annul the effect of gravity as far as this reference system is concerned. At least it would annul it as regards its effect on the weight. Now Einstein makes the assumption that the same transformation of reference systems, which would annul the influence of gravity as regards the weight, would annul its influence as regards everything else.

Time will not permit me to enter into a detailed discussion of the situation, but it will be sufficient to state that the logical consequences of this idea are that, on account of the greater gravitational field at the sun as compared with the earth, the light emitted from an atom on the sun should appear to us of a smaller vibration frequency than that emitted from a corresponding atom on the earth. Here again the difference is only very slight, amounting to about two parts in a million. Nevertheless, although this has been the most difficult conclusion to verify, the evidence obtained within the last year or two at Mount Wilson Observatory is considered strongly in favor of its truth.

There are many aspects of the theory of relativity on which it has been impossible for me to touch in the time at my disposal this evening. Prominent among them is the geometrical significance of the whole theory. I have omitted this because in the first place it is not related to the statement of the working aspects of the theory, and in the second place it involves ideas which when expressed in language have a wondrous sound indeed, dealing as they do with curved non-Euclidean spaces of four dimensions, with finite universes and the like, but which are apt to convey a wrong impression as to their significance unless the language of the description has the meaning to us which only long familiarity with it can give.

One frequently hears the question, "Is the theory of relativity right or wrong?" I do not know that the question has as much meaning as it appears to have. If an English student of chemistry should ask me whether the French language is right or wrong I should hardly know how to answer him, for I should not know exactly what he meant. It might be that the English language was a more suitable one in terms of which to tell of the facts he wished to discuss. It might even happen that these facts were incapable of being discussed in the French language because of

the lack of means to express certain situations in that language; but, I could hardly say that the French language was wrong.

The great difference between the relativity theory of gravitation and the older standpoint lies in the way in which the law is described. Our older ideas present to the mind the picture of a planet moving around the sun in an ellipse because it would really like to move in a straight line, but the sun does not approve. Now the theory of relativity sees the planet moving almost exactly in the same ellipse, but the structure of the whole theory is such that the mathematical properties of this orbit constitute by themselves a more fundamental reason for the existence of the orbit than any picture of an argument between the sun and planet as to how the latter ought to travel in space. The theory sees, in the beauties of a mathematical curve, a more fit field for the expression of Nature's laws than in the doctrine of push and pull. The actual Newtonian law itself, expressed as a mathematical fact, is altered very little, at any rate for velocities not comparable with that of light; and far from detracting from its great importance, the theory of relativity provides in the scheme of Nature's laws a setting for it appropriate to its dignity. Instead of leaving it alone as the guardian of worlds on their journeys through space, it moulds it into an harmonious unity with the whole of Nature, a unity which probably none would have welcomed more than its author, that great prince of the world's men of science, Sir Isaac Newton.

(*Note Added May 10, 1925.*) Within the last three weeks Prof. D. C. Miller has communicated to the National Academy of Sciences the results of his repetition of the Michelson-Morley experiment, in which he concludes that on the top of Mount Wilson a real shift of the interference fringes is to be observed as the apparatus turns through 90°, while no shift occurs when the apparatus is used at his laboratory in Cleveland. A discussion of the conclusions to be drawn from these most interesting results must naturally await publication of the full experimental details; but, it is nevertheless of interest to contemplate what the situation would be if this shift of the fringes remains after all sources of disturbance (in the sense in which the term is usually understood) have been removed. I plan to discuss this matter in some detail in a paper now in preparation. The elements involved

can hardly be brought out clearly in a note added in proof; but, it will suffice to say that full consideration of all the circumstances would make it appear that even a positive result in the Michelson-Morley experiment would not necessarily destroy the most useful features of the restricted theory of relativity.

An Aluminum Centennial.—The discovery of aluminum is generally credited to Wöhler, under date of 1827, but Matignon and Faurholt, in a communication to *Chemie et Industrie* (1925, **13**, 9), claim that the metal was first prepared by Oersted in 1825. Davy, as is well known, had some years before isolated potassium and sodium by electrolysis of their hydroxides, and Berzelius had taken advantage of the high affinity of potassium to decompose silicon fluoride, obtaining silicon in the form of a brown powder converted by chlorine into a volatile compound. Oersted proposed to convert alumina into a chloride and then reduce this with hydrogen, but failed. He then substituted an amalgam of potassium and obtained a reduction which he announced formally to the Danish Society of Sciences. The results were published in the early part of 1825, and a specimen of the new metal was presented to the Society. Oersted described it as having the lustre of tin. The minutes of the Society contain the record of this work, but several letters are also extant and he prepared a short pamphlet, which, so far as known, has not been published. Two years later Wöhler obtained aluminum as a gray powder. It is stated that he had visited Oersted at Copenhagen and from information given was able to prepare chlorides of tantalum, boron and titanium. Wöhler published his work concerning aluminum in 1827. He mentions Oersted's work, but says that the metal was not obtained by the use of potassium amalgam. He used potassium alone. Matignon and Faurholt, however, assert that the metal can be obtained by the amalgam as well as by the potassium itself, and further that the amalgam yields the metal in a coherent form with some lustre, while the other process gives a pulverulent mass. It is therefore claimed that Oersted was the real first producer of free aluminum.

Documents recently discovered by Madam Kirstine Meyer among Oersted's papers show that together with an associate, Köster, description is given of the methods to be used for the satisfactory production of the metal. These documents are about to be published. Among other points it is stated that by employing the amalgam, the reduction can be carried out in a glass tube which is not the case when potassium alone is used. The best results are attained by reducing potassium aluminum chloride. This salt fuses below the boiling point of mercury and permits the globules of free metal to collect readily under the liquid salt. In 1826, Oersted published a

paper in which he described the properties of the metal he had obtained, stating that it had a distinct metallic lustre. He found it also capable of decomposing water, due probably to impurities, especially mercury. H. L.

On the Effect of Temperature on the Anomalous Reflection of Silver. M. DE SELINCOURT. (*Proc. Royal Soc.*, A 742.)—As is well known, light is reflected from silver surfaces, but the fraction of the incident light that is reflected is not the same for all wave-lengths. "The reflection coefficient of silver, besides diminishing steadily in the ultra-violet, shows a marked anomaly in the neighborhood of wave-length 3200 Å., sinking to a value nearly zero, while on either side of this point it amounts to about 50 per cent. The cause of this phenomenon is unknown, though from the classical electromagnetic point of view it would naturally be regarded as some kind of 'resonance' effect; that is to say, that the 'free' electrons cannot, for some reason, vibrate with frequencies corresponding to this region. If this is so, and if the restoring force on the vibrating electrons varies with the distance between them and the neighboring atoms and electrons, it may be expected that the limiting frequency on either side of this band will vary as the metal contracts or expands. A change in the temperature of the silver should then produce a corresponding shift in the position of the band of weak reflection. Lowering the temperature which causes the metal to contract and so brings the atoms closer together, would tend to increase the force, if any ordinary law of force holds, and consequently to increase the proper frequency of the 'resonance.'"

Light from a filament lamp entered a chamber through a quartz window and fell upon a plate of polished silver. The reflected light passed to a quartz spectrograph. To eliminate the visible light a tube containing bromine vapor was placed in the path of the beam. The silver was surrounded by dry hydrogen to prevent the condensing of water vapor inside the chamber with the lowering of temperature. The deposition of moisture and the formation of frost on the outside of the quartz window was avoided by applying to it "methylated spirit." The spectrum of the light that reached the spectrograph was photographed and the blackening of the plate for various wave-lengths was measured photo-electrically. This was done when the plate was at four different temperatures—16°, −183°, −79°, and 150° C. "All the photographs showed that on lowering the temperature the band was shifted toward the shorter wave-lengths. Further, the band was at the same time rendered sharper and narrower." For temperatures of −183°, −79°, 16° and 150° C. the wave-lengths for minimum reflection were found to be 3217, 3227, 3236 and 3247, respectively. Except for the highest temperature the minimum is very sharply marked. For a wave-length of about 3205 Å. silver at the three lowest temperatures has the same reflecting power. G. F. S.

SOME FUTURE PROBLEMS IN ENGINEERING.*

BY

WILLIAM BARCLAY PARSONS, C.E., LL.D., Sc.D.

THE so-called learned professions were originally confined to three—law, medicine and theology. The study of science and its application to human affairs have added at least one more, engineering, to such a group. But while the first three are venerable with age, the last is still a youth exuberant with hope for a long life of growth and development. What may be some of the features of that growth, with the attendant difficulties and problems, form the theme of this evening's address.

The creations of engineering surround us so closely on every side, they are so intimately related to our very existence that we are apt to accept them as matters of course and forget how very young the profession is. Modern civil engineering, using the term "civil engineering" in a broad and generic sense, is a product of the nineteenth century. The combination of words was used for the first time by Smeaton in 1786, but it was not recognized as a part of the English language by Todd's Dictionary, the accepted authority of 1818.

The year 1818 was, in many respects, a counterpart of the period exactly 100 years later. The Napoleonic wars that had devastated Europe were at last ended, but leaving in their trail blasted development, depreciated currencies, the flower of the young generation dead, and all nations weighted with masses of debts from under which there was little hope that they could ever emerge, except through repudiation. The color scheme of the world picture that we view to-day is not unlike that of 1818.

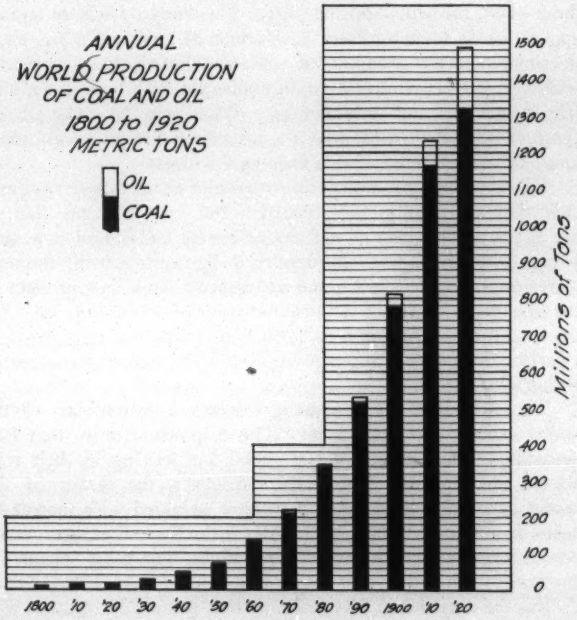
But the engineer in that year, whether or not his vocation was defined, was already at work. The steam engine was in existence, the *Claremont* was running on regular trips, and in 1825 the Stockton and Darlington Railway was opened. Another era had dawned. Through steam, a new source of power, the productivity of man was increased, and with it his ability to consume also grew. Commerce was revivified, new channels for industry were created, agriculture was stimulated, financial credit and confidence

* Presented at a meeting held Thursday, April 2, 1925.

were restored, and national debts were soon reduced to manageable proportions.

Man's progress has never been made through uniform movement but rather by a series of impulses, separated by periods when forward development was scarcely visible if there was not

FIG. 1.



actually a retrogressive movement. The social and economic structures that men from time to time have created for themselves, and through or under which they have made progress, we euphemistically call "Civilizations." Thus we speak of the several civilizations of the ancient peoples, or that of the Renaissance. Each of these civilizations had its inspiring motive, each ran its course to its own fulness, and finally succumbed, in most

cases apparently to outward influences, but really from that inward disintegration that inevitably follows completion of growth.

When at some future epoch, long postponed I hope, the then historian will make an analysis of our times, how will he describe our civilization, for certainly our social and economic structure is so strikingly different from that of the eighteenth and seventeenth centuries that separate it from the last recognized civilization, the Renaissance, as to entitle it to a special designation? The characteristics that distinguish the present epoch from any of its predecessors, is the substitution of mechanical for animal power, and the application of mechanical means to secure results obtainable in no other way. Whatever the name that will be applied, it does not stretch our credulity to imagine that it will be some word expressing dynamic energy, and that our economic structure will be referred to as the period of Applied Mechanics or Power.

Our economic structure is dependent absolutely on mechanical energy. Take that away and our much-vaunted civilization would go to pieces, just as surely as the civilizations of Egypt or the Roman Empire passed away. The energy that we use and on which our civilization rests comes almost wholly from the burning of fuel, coal or oil; that produced from falling water, though large in amount, is proportionately small. Figures of reasonable accuracy showing the world consumption of fuel are available and are plotted in Fig. 1. This diagram and the table giving the details show the number of metric tons of coal and oil produced during the year ending each decade from 1800 to 1920, both inclusive. In the diagram the shaded portion shows the amount of coal produced and the upper white area that of oil, the figures for oil, usually stated in barrels, being reduced to metric tons by rating 4.5 barrels of oil as equal in heat-giving capacity to 1 metric ton of coal.

From the figures of Table I it will be seen that at the end of every one of the thirteen decades there was an increase in production over the year ending the previous decade, and, with the single exception of the decade ended with 1920, such increase was greater than that of the preceding decade. This increase in ratio increase is strikingly shown in the diagram. The failure of the increase from 1910 to 1920 to exceed that of 1910 over 1900 is probably due to the dislocation of industry following the Great

War, and to some extent to economies effected in steam production and the increase in hydro-electric power output. If the increase in coal production in 1920 over that of 1910 was less than the increase of 1910 over 1900, nevertheless, the increase in oil production was continuously greater for every decade from its first production in 1860.

In 1920 the coal reserves of the world were estimated to be about 7,300,000 millions of tons, by no means all of which, however, will be recoverable. The oil reserve is unknown except that its amount is distinctly limited and will be exhausted, or at least seriously reduced, in the not very distant future. If we were to continue to produce fuel at the present rate without further increase, the supply would last nearly five thousand years, but our rate of consumption has doubled in the last twenty-three years, had doubled in the sixteen years prior to that period, and again doubled in the previous twenty-four years. Assuming that in the future the consumption of fuel will double in periods of thirty years, a not unlikely result, for as modern civilization spreads among the densely populated parts of the world millions of people now dependent on hand labor will institute new demands for mechanical energy, the total reserves will be exhausted in 217 years. Even should the rate of increase be reduced to doubling in fifty years, the reserves would be entirely used up in 325 years and the world be left without a

TABLE I.

World Production of Coal and Oil, Expressed in Millions of Metric Tons (2204 Pounds).

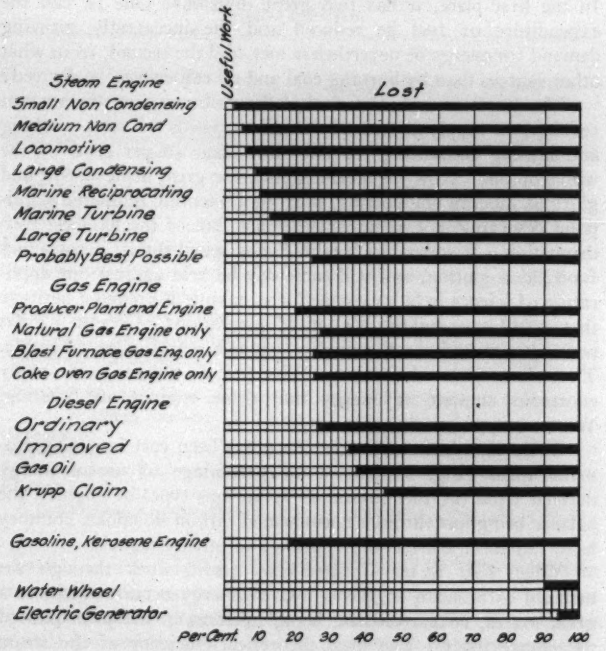
Year.	Coal.	Oil.	Total.	Increase over Previous Decade.
1800	14.1		14.1	
1810	16.3		16.3	2.2
1820	19.6		19.6	3.3
1830	27.1		27.1	7.5
1840	49.6		48.6	21.5
1850	75.1		75.1	26.5
1860	139.0	.1	139.1	64.0
1870	217.0	1.3	218.3	79.2
1880	336.0	6.7	342.7	124.4
1890	515.0	17.0	532.0	189.3
1900	778.0	33.1	811.1	279.1
1910	1165.0	72.8	1237.8	426.7
1920	1319.0	154.5	1473.5	235.7

pound of fuel, except what can be grown annually. As consumption cannot continue unchecked to the moment of exhaustion, the rate of production will begin to decline long before the

FIG. 2.

DIAGRAM

Showing proportions of Potential Energy converted into useful work and lost by various types of engines.



end is reached. Our state of civilization being founded on energy, when the demand for more and more energy cannot be met, decline will then begin, and the end of our form of civilization will be in sight.

The civilization of the nineteenth century, under which we are

still living, is the product of engineering. The great future problem facing engineering to-day is, therefore, how can the vital force be kept growing, for how long can the catastrophe be postponed, or is the science of engineering helpless to aid and keep alive the movement that it set going? Are we to contemplate that in the short space of not exceeding 100 years our civilization will be on the down grade through forces acting from within?

This problem is not a single one, but a grouping of many. In the first place, it has two great divisions: One is, can the expenditure of fuel be reduced and the incessantly growing demand for energy be nevertheless met, and the second, from what other sources than by burning coal and oil can energy be derived?

The great consumer of fuel is the steam engine, the amount required to supply the strictly domestic needs of man, cooking and heating accounting for not more than 10 per cent. of the whole output. Now the steam engine, the great achievement and glory of modern engineering, is from a scientific economic standpoint deserving, not of unstinted praise, but of the harshest condemnation. It squanders wealth like a spend-thrift, it consumes food like a glutton, and will some day be held against our application of science in bitter complaint as having destroyed a heritage that would have sufficed for untold generations. Go into a great power-house and see the mighty engines working so smoothly. They excite your admiration. What you do not see is the really enormous amount of energy that those engines are wasting. Waste, not power, is their output.

Table II, which for convenience has been cast into diagrammatic form (Fig. 2), shows the percentage of useful energy derived from the fuel consumed by various types of engines, the balance being lost through unconsumed carbon in smoke, chimney heat, radiation, convection, friction and other channels.

When heat is transformed into useful work through the medium of a steam engine, a loss of energy occurs which, to a great extent, is unavoidable, being inherent in the principles of the engine itself. The ideal theoretical efficiency of the steam engine, as determined by the French scientist, Carnot, a hundred years ago, is proportional to the difference of the initial and terminal temperatures of the steam, and inversely proportional to the initial temperature (absolute). The limits of both generating and condensing temperatures are fairly well defined. At a tem-

perature of 750° F., the best steel and castings used in plant construction begin to show marked decrease in tensile strength and sustain objectionable deformation under the high pressure necessary with water steam. This marks, therefore, the practical upper limit of steam generating temperature with our present knowledge of steel metallurgy. The steam pressure corresponding to a steam generating temperature of 750° F. is over 4000 pounds,

TABLE II.

Engine Efficiencies.

Type of Engine.	Energy in Fuel.	
	Per Cent. Converted into Useful Work.	Per Cent. Lost.
Steam Engine.		
Small non-condensing engines	2.5	97.5
Medium non-condensing engines	4 to 6	96 to 94
Locomotive	5 to 6	95 to 94
Large condensing engines	8 to 10	92 to 90
Marine reciprocating	10	90
Marine turbine	13	87
Large steam turbine	14 to 19	86 to 81
Best considered possible with steam as medium	25	75
Gas Engine.		
Gas producer plant and engine	18 to 26	82 to 74
Gas Plants When Gas Is Already Made		
Natural-gas engine only	27	83
Blast-furnace gas engine only	25	75
Coke-oven gas	25	75
Diesel Engines.		
Ordinary	26	74
Improved	35	65
Gas oil	37	63
Krupp claim	45	55
Gasoline and Kerosene Engines.	17 to 20	83 to 80

a value far exceeding the present limits of design. The maximum steam generating temperature disregarding superheat and pressure realized in actual practice in large plants are 570° F. and 1200 pounds per square inch. The final terminal pressure and temperature are fixed by the temperature of the condensing medium. In actual practice the temperature of the available water for condensation will average about 70° F., which gives an equivalent terminal pressure of .359 pound per square inch absolute. Assuming the foregoing limits of 570° F., initial, and 70° F.,

final temperature, the theoretical Carnot cycle efficiency is 48 per cent. The Carnot cycle represents the ideal theoretical efficiency of heat engines. Allowing for the less efficient cycle of actual heat engines and deducting for losses of boilers, stack, radiation and other plant losses, the best obtainable plant thermal efficiency is about 25 per cent.

Other substances might be used in conjunction with water steam to extend the range of temperatures without unduly high pressures and increase the thermal efficiency. For example: Mercury at 15 pounds gage has a temperature of 730° F. Mercury vapor may be generated in a mercury boiler, used in a mercury turbine and condensed so as to generate water steam in its condenser. This water steam may then be used in a steam turbine in the usual way. This process, as it now seems practicable, shows an apparent gain over water steam alone of about 35 per cent., but all such expedients present new difficulties of their own that may offset the gain in large part, if not entirely.

That modern engineering science can produce a machine that can work without serious waste of energy, we have but to consider the modern water-wheel which will deliver ready for useful work 90 per cent. of the energy in the falling water, while the electric generator will return more than 96 per cent. of the actuating energy that it receives. For comparison these figures have been placed on the efficiency diagram.

The fact remains that if fuel production is to be radically reduced so that the energy of the sun stored during the carboniferous and other long-past epochs is to be saved for man's use for an indefinite number of centuries, the steam engine as a prime mover must be supplanted by some different form of device whereby, say, 80 per cent. of the energy in fuel may be converted into useful work and the loss reduced to 20 per cent., the reverse of the best practice of to-day. What that type of device will be is the first great problem of engineering and is one that should not be unsolvable. It is far more likely of accomplishment than a high-pressure compound-condensing steam engine would have appeared to be in 1825. Success, however, will not lie in improvements on existing methods, but in a complete departure from them along some line quite as radical as was the steam engine when first developed.

Before passing to the many problems that compose the second

grand division of saving fuel, namely, the production of energy by other means than by burning coal or oil, consideration must be given to the problem how to conserve or store energy until needed, for this is indissolubly linked with the successful solution of production. The sources of energy that give the best promises of economical development produce intermittently or irregularly and at intervals that are not consonant with the demand, or they are located geographically where there neither is nor ever can be extensive local consumption. At present the only means of storing power are reservoirs connected with hydro-electric plants and the electric storage battery. Such reservoirs may be either natural like the Great Lakes that equalize the flow of the Niagara and St. Lawrence Rivers, or artificial, specially constructed to hold back floods to be drawn later when needed. There are very few illustrations of the natural type, and not many opportunities to realize the artificial. The reservoir will be only an occasional and therefore partial solution of the problem. The electric storage battery is cumbersome, expensive and highly inefficient. It is nothing more than a temporary regulating medium.

To store power economically and on a large scale, recourse to the chemical engineer will probably be necessary. Then the problem will be whether he can devise a method whereby power can be stored more efficiently than at present. For illustration, two or more substances might be produced, each inert when alone, but which when brought together will set energy free, exactly as the constituents of gunpowder do upon the transmission of an electric spark, the energy taking some form of electrical vibrations or radio-activity susceptible of direct use without the intermedium of an uneconomical device such as the steam engine. Such substances should be of a character that would not be adversely affected by time or by changes in weather or temperature, and be capable of being stored and transported like coal, or the energy developed and transmitted by wire, or perhaps even by a directed wireless arrangement to the point of consumption. Or another solution might be the disassociation of a compound into its constituent elements such as water into hydrogen and oxygen that on reuniting would generate energy.

The successful solution of this problem would at once make available hydro resources not now possible of economical development. Except a few rivers like the Niagara and St. Lawrence,

all streams have a very variable and highly irregular delivery. The Susquehanna River, for example, has been measured to deliver from less than 2500 feet per second to a flood flow estimated at more than 700,000 cfs.

Without a steam reserve it would not be economically feasible to develop such a stream much beyond the minimum flow, according as pondage would suffice to help meet the daily load factor demand. With the problem of storing power solved, any stream could be developed to save much of the energy contained in floods, now wholly lost even with a steam reserve.

It is estimated that there are 439,000,000 potential horsepower in the world, of which 190,000,000 are to be found in Africa. About 10 per cent. of the total world power has been developed. These figures show the extent of undeveloped resources and the large proportion to be found in localities undesirable for habitation and field of work by the white race.

The interrelation of the effects produced by the solving of these problems is extremely interesting. Hydro-electric developments are economically feasible to-day because in spite of their high fixed charges on plant and transmission lines, their operating costs are so low that the final total cost per kw.h. is less than that of the same unit when produced by steam. But should an engine be devised whereby a quarter or perhaps a fifth of the coal now required could be made to produce the same result in power output, it is obvious that hydro plants would cease being developed except under specially favorable conditions. On the other hand, given the means of storing energy, thus permitting hydro plants to be developed on the basis of flood conditions, they would again become successful competitors against fuel-produced power even if the efficiency of the latter plants were much increased.

When energy can be stored and transported the great sources of hydro power existing in the tropics, where the rainfall is the maximum and heads high, now untouched through entire lack of local demand, will become at once available for development. Their energy output in stored form could be carried to those other parts of the globe where climatic characteristics are such as men can live and work under reasonably comfortable circumstances. But assuming that all the hydro possibilities, even under the most advantageous conditions, were developed, and that the torrid zone were making its contribution, the ever-increasing demand for

power would not be met. Should all the falling water be utilized, the combined product would not suffice to keep industry going on our present scale. New York State, for example, is the most favored state in the Union in regard to potential hydro power, including its share in the Niagara and St. Lawrence Rivers. Should these two streams be developed so as to produce the maximum amount of power, which would mean the complete destruction of Niagara Falls as a scenic asset, and the other streams within the state be similarly treated so as to utilize flood conditions, perhaps 5,000,000 hp. would become available. This would mean the saving of 50,000,000 tons of coal at the present basis of power production per ton. But there is consumed within the state at the present time nearly 60,000,000 tons annually. New York's potential hydro power, and it is larger than that possessed by any other state, would not suffice to offset its fuel demand.

The world must therefore turn to other possible sources of energy, and here we enter the second grand division of the main problem as laid down at the beginning of this address, and this field presents nothing but unsolved problems.

Among sources of power other than fuel and the weight of falling water are: Atomic energy, internal heat of the earth, tides, waves, wind and solar radiation. None of these sources can be developed successfully without first finding some means of storing and transporting the energy when produced, because their rate of production will not harmonize with that of consumption.

Atomic energy, the heat of the earth, tides and waves do not offer much hope at present. As to atoms giving results, Dr. R. A. Millikan, the leading authority to-day in this line of investigation, writes me:

"We are indeed in this laboratory endeavoring to discover whether any new sources of energy can be expected from the stimulation of sub-atomic transformations of any sort, but at present we have no hopes to hold out for this sort of substitute for coal, and I think that there is an increasing opinion among physicists that there is at least no immediate likelihood of this type of solution of our energy problem. It is of course worth while to continue experiments in these directions, but the only supply of energy which we can count upon is the well-nigh unlimited supply which is coming to us from the sun. The utilization of this in new ways—and ways some of which can be made

commercially feasible when coal is a little less cheap—is after all our great problem."

As this address is intended to be confined to the field of problems that are probably capable of solution and avoid those that are in the realm of speculation, further consideration of energy to be derived from the atom will be put aside, although men of outstanding scientific reputation continue to be engaged in its examination. A similar conclusion can be reached regarding the utilization of the internal heat of the earth. Plans have been proposed, but they are of such a fantastic nature as not to bring this question within the scope of engineering. Tides and waves appear to give ocular evidence of great forces continually at work, but their efforts are so diffused as to render them of doubtful application, except in the case of tides in a few localities, such as the Bay of Fundy, where owing to peculiar topographical conditions a tidal rise occurs of extraordinary height. Along most of our coasts the range is less than three feet. On the Gulf and Atlantic seaboards it is not until the tidal wave passes north of New York that it has a greater mean height than five feet. The maximum theoretical energy possessed by such a wave is that due to the water falling through the tidal height. An acre of water falling five feet in six hours, the tidal period, would develop only 17 hp.h. during the time of tidal ebb, but as another period of six hours would be needed to restore the head, only one-half of that amount would be theoretically realizable on every complete tidal cycle. Great as is the energy existing in the tidal wave when the surface of all oceans and seas is considered, it is spread over such an enormous area as to possess but a small amount of power when any unit of reasonable size is considered. Possibility of deriving power from tidal flow is therefore limited to a few localities, but even the most exceptional cannot produce enough energy appreciably to affect the world's supply.

The dynamic energy of breaking waves, while impressive in appearance, is of moment only during storms, which are active but a small portion of total time. Under such conditions there are obviously great mechanical difficulties in the way of utilizing the latent energy. Tides and waves may therefore be put in the same category as the internal heat of the earth, all possessing a

great amount of latent force, but so diluted as to offer little prospect for extensive recovery.

Wind possesses considerable potential, some of which is recovered to-day, and more could be recovered if the need to do so were sufficiently pressing. There are many places in our country where the wind will average eleven miles per hour, throughout the year. At this velocity a square metre at right angles to the breeze represents theoretically about one-tenth of a horsepower. An area 10 metres high and 100 metres long would represent 100 hp. The mechanical difficulties in keeping the device always facing the wind are so great that the idea does not offer much encouragement except for small isolated plants. However, with the storage problem solved, wind in the tropics, where the trade winds blow steadily at fairly high velocity and with an almost constant direction for long periods, might render possible specially designed plants operated by wind that would generate power on a large commercial scale.

Solar heat is on a different footing, for it is the great source of all our power. It created in other geological ages our coal deposits, storing in them energy that we are releasing so wastefully. It grows the crops on which we feed, it maintains animal life and is the great pump that lifts water from the sea to be precipitated as rain.

The furnace that produces all this energy has a diameter of 863,600 miles, and a surface exposed to the earth, if projected as a flat plane, of an area of 585,750 millions of square miles, 10,000 times that of the earth. As the temperature of the sun is estimated to be 6000° C., each square foot of the above enormous area radiates heat sufficient at full efficiency to generate 12,500 hp. This energy is scattered throughout space and, so far as we are concerned or know, is nearly wholly lost, only an infinitesimal part of the whole striking the earth. However, 7.12 B.T.U. per sq. ft. fall on the outer atmosphere per minute, of which about 70 per cent., or 4.98 B.T.U., reach the earth's surface at tide water on a perfectly clear day, the balance being absorbed by the atmosphere. On mountain tops the amount of heat received is greater, being 87.3 per cent., or 6.22 B.T.U., on the summit of Mount Wilson (6000 ft.) and 6.43 B.T.U. on Mount Whitney (14,500 ft.).

4.98 B.T.U. per minute give 299 B.T.U. per hour. Since 2546 B.T.U. per hour are the equivalent of one horsepower, an acre of sunlight during the noon hour on a clear day at sea level would furnish theoretically 5109 hp.h. Here is a stupendous amount of energy going to waste. Assuming the total span of daily sunlight to be equal to nine hours of maximum intensity, and assuming that 50 per cent. of the energy be recoverable, a strip of land 10 miles wide and 100 miles long would develop daily 14,700 million hp.h. This volume of power would require annually for its production by steam at three pounds of coal per hp.h., a moderate allowance for an average when all kinds of engines are covered, not less than 7300 million metric tons of coal, or one-half of the total consumption of fuel in 1920. As the desert of Sahara, much of our own country lying between the Rocky and Sierra Nevada Mountains, where the atmospheric conditions are better, and other parts of the arid portions of the world are fit for no other useful purpose, there is an enormous area ready to meet our demands when the problems of recovery and storage of energy have been solved. In fact, on the above basis of efficiency the area of Salt Lake, Utah, alone would suffice to produce energy equal to that furnished by the whole annual world consumption of fuel.

Scientific men have been trying for centuries to devise means to convert solar energy into useful work. Setting aside as more or less mythical the stories of early efforts, it is known that the problem has been under serious investigation from the time of Solomon de Caus in 1616. Of recent experimenters, Ericson, of *Monitor* fame, added much to our knowledge, while Mr. Frank Shuman in 1910–1911 erected a plant at Tacony, near Philadelphia, which actually produced power. This plant had 10,296 square feet of absorbing surface, so that it was much more than a laboratory experiment. The device consisted of a system of reflectors concentrating sunshine in the ratio of 2 to 1 on a hot box in which a temperature of 250° F. was obtained. Steam was generated and a maximum thermal efficiency of 29.5 was reached, a highly encouraging figure.

In 1913 a similar plant was erected at Meadi, in Egypt, under the direction of Messrs. Ackerman, Walrond and Boys. The last, a physicist, suggested several improvements in design so that the new device was called the Shuman-Boys absorber. It was larger

than its Philadelphia prototype, the absorbing surface presenting 13,269 square feet. The reflectors, of parabolic cross-section, were placed with their axes running north and south, and by appropriate mechanism followed the passage of the sun so that the open parabolæ were always turned directly sun-ward and the rays were always concentrated at the focus where the boiler was located. The ratio of concentration was 4.5 to 1. Twelve pounds of steam were produced per 100 square feet of absorber, equivalent to 183 square feet per brake horsepower in the particular low-pressure steam engine employed. The maximum thermal efficiency was 40.1 per cent., a distinct advance over Tacony. It was found that the maximum, minimum and average output were in close agreement, the maximum exceeding the average of five hours by only 6 per cent., while the minimum hour fell short by less than 12 per cent. The maximum over-all efficiency of the Shuman-Boys absorber was 4.32 per cent., a result that compares favorably with certain types of steam engines.

Both Shuman and Boys attempted to convert the latent solar energy into useful work through the intermedium of aqueous vapor. They had no other recourse. They were compelled to use an engine operated by very low steam pressure, the most inefficient type of the steam engine. The ratio of 4.32 per cent. is under the circumstances surprisingly good and highly encouraging. Many writers commenting on these experiments speak of the poor result, but in so doing they miss the point of the experiment and fail to grasp the lesson of actual accomplishment. Shuman and Boys undertook to convert solar heat into energy. That they realized such a small proportion as 4.32 per cent. of the total received was due, not to defects in their apparatus nor in any unsoundness of the theory underlying their work, but to the very low efficiency of the steam engine and particularly of the type they were compelled to use. Their critics have not allowed due weight to what they accomplished nor to the inescapable limitations of the only tool they had at hand. Experiment and criticism alike, however, bring out clearly certain indisputable facts: Solar energy exists in enormous quantity and is recoverable, but the steam engine is an impossible means of doing so from the point of view of efficiency. To solve the general problem and this particular phase we must free our minds of existing methods and begin to think in new terms. This last statement, if not axiomatic in that

its truth is self-evident but possibly remains to be proved, may well be descriptive of the road that leads to a solution. Success will not lie in changing water into vapor by heat, but by finding some way whereby energy can be converted directly into power or in making substances that will produce energy. Nature does it in the leaf of every plant. By solar heat, CO_2 is disassociated, some oxygen set free, and the balance converted into carbohydrates, the basic elements of fuel. As usual the process is inefficient, but Nature is always wasteful. She has, however, pointed the way for man to follow and to surpass her.

Engineering has created our civilization. Will the engineer of the future, calling physics and chemistry to his aid, be able to supplant the steam engine with a more efficient power-producing device and to store energy until such time as he will need to convert it into work? Or will he be helpless to save his own creation by failing to feed it with the power so necessary for its existence? Those are the future problems of engineering. Everything that we value to-day depends for existence upon their solution. If the past be the criterion by which the future may be judged, then the achievements of the young profession in the first century of its existence provide a foundation for a reasonable confidence that success will be achieved.

Ethylene.—The manufacture and uses of ethylene are described by F. B. ARENTZ (*Jour. Chem. Education*, 1925, **2**, 459–462). It is made by the dehydration of ethyl alcohol, passing the vapor of the latter compound over a suitable catalyst. The purified gas is compressed into cylinders containing from two to twenty-five pounds. The chief uses of ethylene are in cutting and welding, the manufacture of organic compounds such as ethylene bromide, the coloring of citrus fruits, and as an anæsthetic.

Ethylene bromide and lead tetraethyl are constituents of ethyl gas, a motor fuel. Oranges and lemons are mature and palatable while still green. The taste may become insipid if the fruit be permitted to remain on the trees for several weeks until the yellow color develops. However, the organoleptic properties remain unaltered, and a yellow color develops in a few days if the fruit be picked while green, and exposed to an atmosphere containing as little as one part of ethylene in 3000 parts of air. Mammals are anæsthetized if the inhaled air contain ethylene in high concentration.

During the late war, ethylene was used in the manufacture of mustard gas. J. S. H.

POLYCRASE FROM BRAZIL.*

BY

FRANK L. HESS and E. P. HENDERSON.

SEVERAL months ago, Mr. James A. Stader, of the Mineral Division of the Bureau of Foreign and Domestic Commerce, called the attention of one of the writers (F. L. H.) to a deposit of euxenite in Brazil and furnished specimens of comparatively fresh material obtained through the courtesy of the firm of Castro, Lopez, and Tebyricá. The deposit seems to be unusually large, for minerals of the euxenite-polycrase group are ordinarily found in comparatively small pieces sparsely distributed through pegmatite dikes. Uranium minerals have been found at few places and in small quantities in Brazil or elsewhere in South America, so that the occurrence is of considerable scientific interest.

Details concerning the occurrence, unless otherwise credited, are taken from a report furnished by the firm named above. The deposit is on the Fazenda de Santa Clara, a coffee plantation 11 kilometres (7 miles) from the small village of Tocantins, on the Leopoldina Railway, 300 kilometres (188 miles) by rail, slightly east of north from Rio de Janeiro. Tocantins is about in latitude 21° 10′ S., and longitude 40° 45′ W.

The deposit is on an outlying hill of the Itatiaya-Assu Mountains, which rises 110 to 120 metres (360 to 400 feet) above the stream at its foot and apparently owes its form largely to a pegmatite dike, which occupies its crest for a distance of a kilometre (five-eighths of a mile). The country rock is a much decomposed gneiss invaded by diorite dikes, and is mapped by Branner [1] as Archean.

The pegmatite is considerably decomposed and is commonly covered by 4 to 6 metres (13 to 20 feet) of earth. Chunks of quartz and potash feldspar are found along its course. More than 15 tons of euxenite-polycrase have been mined, and pieces weighing more than 16 kilos (35 pounds) each have been taken

* Communicated by the authors and published by permission of the Director of the U. S. Geological Survey.

[1] BRANNER, JOHN C.: *Geol. Soc. Amer. Bull.*, 30, No. 2, June, 1919.

out. Some of the material shows more or less crystal form. The mineral is accompanied by white and smoky quartz, muscovite, fuchsite, and a little well-crystallized euxenite.

The specimens received were fragmental, about an inch thick and two inches across, semi-translucent, and of a dark reddish-brown color. Old surfaces were covered with a thin coating of a light greenish-yellow decomposition product somewhat resembling serpentine. Clear mineral, showing none of the alteration product, was analyzed with the results given in Table I.

TABLE I.

*Analysis of Polycrase from Tocantins, Brazil.**

Columbium pentoxide	Cb_2O_5	20.31	}	
Tantalum pentoxide	Ta_2O_5	?	} †54.72	
Titanium dioxide	TiO_2	34.41	}	
Yttrium earths $(Y, E)_2O_3$	}			
Cerium earths $(Ce, La, Di)_2O_3$	}		29.28	
Thorium oxide	ThO_2		5.22	4.59 Th
Uranium oxide	U_3O_8		6.48	5.49 U
Lead oxide	PbO		0.64	0.59 Pb
Ferric oxide	Fe_2O_3		0.78	
Aluminum trioxide	Al_2O_3		0.44	
Water	H_2O		2.04	
			99.60	

* By E. P. Henderson.
† See text following.

All the determinations except those of columbium, tantalum, and titanium are within the limits of experimental error allowable in refined quantitative methods. The oxides of these three metals were weighed together and the total weight of these oxides is given as a good determination.

The value for titanium separately was obtained by using the colorimetric method. As this method gives nothing more than an estimation for large quantities of titanium, the difference obtained by taking the estimation of titanium from the total weight of the three oxides is given as an estimation for the combined oxides of tantalum and columbium, and not as a direct determination.

Corrections were made for the volume that titanium occupies in the combined oxides of the three metals by using the following method: Titanium being estimated as 34.41 per cent., one gram of these oxides would contain 0.3441 gram of titanium oxide and

0.6559 gram of columbium and tantalum oxides. A value of 4.07, which as an average of seven values given in different determinations reported, was taken as the density of chemically prepared titanium oxide. The volume was calculated by using the formula $V = \frac{M}{D}$. Then letting V' represent the volume of the three oxides (by pycnometer), $V' - V = V''$ gave the volume of columbium and tantalum oxide. With a value for V'', the density of the combined oxides of columbium and tantalum was obtained by the formula $D = \frac{M}{V''}$. The value of M in this case is 0.6559, and the formula gives a density of 4.54 for the mixture of columbium and tantalum oxides.

An interpolation was made by means of a graph, the density of columbium oxide being taken as 4.5 and tantalum oxide as 8.7. It is evident that a density of 4.54 indicates only a small quantity of tantalum, if any. Though the values for these three oxides may not be individually exact, their sum is believed to be accurate.

The classification of this mineral did not depend upon an accurate determination of each of these constituents, but upon their ratios, and for this reason quantitative methods of determining these elements that would require more time were not used.

The Bureau of Standards examined spectroscopically a part of the selected material for germanium, scandium, and zirconium, but found none. This examination, however, showed a trace of tin.

The index of refraction was determined by Dr. C. S. Ross as 2.248.

In this analysis the molecular ratio of $Cb_2O_5 : TiO_2$ is 1 : 5.5.

The mineral has usually been called euxenite, and its color is that familiar in euxenites, but the high molecular ratio of titanium oxide to columbium oxide throws the mineral into the polycrase end of the euxenite-polycrase series.

Accepting tentatively the statement that lead forms from uranium and thorium at rates represented by the formula:

$$\frac{\text{Pb}}{\text{U} + 0.357\ \text{Th}} \times 66 \times 10^6 \text{ years} = \text{age},$$

then an age of 546,000,000 years is given to the mineral, which,

compared with the estimations on other radio-active minerals, would seem to throw the origin of this mineral into the Devonian.

It is possible that the quantity of lead shown by the analysis may be low; that the pegmatite may have been intruded into the country rock long after the rock had acquired its gneissoid structure; that the method of estimation is erroneous; or that the ratio of lead formation from uranium compared with that from thorium is somewhat in error, but by constant attacks the problem of the age of uranium and thorium minerals may be solved and from them the age of the rocks in which they occur, and this analysis is presented in the hope that it may contribute toward that end.

Ethyl Perchlorate.—CHARLES E. MUNROE (*Explosives Eng.*, 1925, **2**, 426–427) calls attention to the fact that ethyl perchlorate, one of the most violent of brisant explosives, was discovered in Philadelphia by Martin Hans Boyé and Clark Hare, working in the laboratory of Robert Hare. Their discovery was reported December 4, 1840. While wide recognition is given to the discovery of various explosives, such as nitroglycerine and nitrocellulose, by foreign chemists between 1830 and 1850, unfortunately this brilliant research of Boyé and Hare has been overlooked by writers on explosives.

J. S. H.

John Griscom.—DR. EDGAR F. SMITH has written a most interesting brochure on the life and labors of "John Griscom, 1774–1852, Chemist" (Philadelphia, 1925, 27 pages, with portrait of Griscom). Griscom taught chemistry by the laboratory method to school pupils at Burlington, N. J., during the closing years of the eighteenth century. He delivered lectures on chemistry, of the university extension type, in New York City, about 1817, and held the professorship of chemistry in the medical faculty of Queen's College, now Rutgers College.

Griscom contributed the abstracts of a chemical nature to the early volumes of the *American Journal of Science*. About 1826, for several years he arranged and supervised the proof sheets of the JOURNAL OF THE FRANKLIN INSTITUTE. He introduced cod liver oil into the materia medica. In Philadelphia, his friends were James Woodhouse, Robert Hare, and Alexander Dallas Bache.

In 1823, Griscom published a book in two volumes entitled "A Year in Europe," in which he described, in an entertaining and instructive manner, his travels of 1818–1819. He relates his experiences with the eminent scientists of his day, such men as Dalton, Davy, Henry, Wollaston, Berzelius, Orfila, Cuvier, Berthollet, Gay Lussac, Haüy, Thenard, and Vauquelin.

Griscom did not engage in research, but was a great teacher and expositor of chemistry.

J. S. H.

EYE FATIGUE AND ITS RELATION TO LIGHT AND WORK.*

BY

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THE practice in industrial lighting has been toward higher levels of illumination as usage proved the advantages to be gained in this direction. This trend towards the use of intensities of illumination that are relatively "high" with respect to what has been customary in the past, had introduced new factors into the problem of lighting, and the question of "eye fatigue" has been given attention.

In the following discussion an attempt has been made to determine the effect, if any, of intensity of illumination upon the eye by controlled laboratory experiments. Discussions are available in the literature detailing the use of high levels of illumination,[1] but these do not mention eye fatigue as resulting from the use of the higher intensities.

The question of the exact nature of "fatigue" is at best a complex one about which much remains uninvestigated and uncertain. It is not the purpose of this paper to discuss the general subject of fatigue either from the physiological or the psychological viewpoints, but rather to limit its scope to the effect produced by visual operations under several intensities of illumination. It is necessary, because of the limitations of the laboratory, to further limit the problem to the comparatively short-time effects of visual work.

In order that the above conditions be observed, any sort of laboratory test must be so devised and controlled that the following definite requirements are satisfied.

(1) The test and test-object must be so designed that the results obtained are produced by some change in the mechanism of the eye and can be attributed only to the tasks performed by the eye under the several intensities used.

* Communicated by the Director of the Laboratory.

(2) Since the test is based upon the effect of "high" and "low" intensities compared with one another, means must be provided to control the amount of work done in each case.

(3) The work done in both cases must be the same in order that the results noted from the test can be ascribed, without mathematical treatment or correction, to the effect of the intensity of illumination. The test-object must therefore be so selected that its visibility is not affected by the changes of brightness within the range used.

(4) A measurable amount of "fatigue" must be produced in a time short enough to make the method practicable for laboratory use.

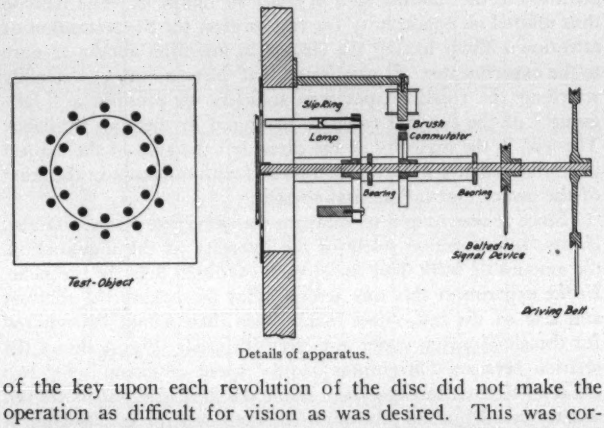
The test-object consisted of a white disc with twelve equally spaced holes near the rim, mounted at a distance of three metres from the subject and so arranged (Fig. 1) that it could be revolved at any desired speed. Surrounding this rotating disc were twelve holes of the same diameter drilled in a white and stationary background. Since the holes were viewed against a darkened room the contrast between them and the flat white paint of the test-object was a maximum. The subject was instructed to follow with his eyes a given spot (master spot) on the rotating disc and at intervals, when this spot was in a radial line with a designated one of the stationary spots, a key was to be pressed. If the eye of the subject succeeded in following the master spot (all spots were of the same appearance), and if the key was pressed at the proper time, then, by means of commutators, electric circuits were completed that operated two counting devices. One counter operated each time the key was pressed; the other operated only when the circuit was completed at the proper time. Thus one counter determined the number of trials made and the second recorded the times the subject was successful in pressing the key at the right time.

The operation of the key lit a small lamp which travelled with the rotating disc directly behind the master spot. This enabled the subject to re-locate the spot in case he had failed to follow it. The disc was rotated continuously and no time was allowed between operations for recuperation. The master spot remained illuminated as long as the key was held down. The brush bearing

on the commutator (Fig. 1) was lifted, however, when the key was pressed by the subject unless it happened to be on the commutator segment at that instant and consequently allowed current to flow through the circuit operating the second counter; when the commutator segment left the brush the latter automatically lifted and thus prevented the second counter from operating when the key was held down continuously.

It was found that the following of the spot and the pressing

FIG. 1.



Details of apparatus.

of the key upon each revolution of the disc did not make the operation as difficult for vision as was desired. This was corrected by arranging an audible signal to occur after the disc had made about three revolutions and instructing the subject to operate the key only after hearing the signal. The ratio was made slightly more than three to one in order that the signal should not invariably occur when the master spot was in the same position with respect to any stationary spot.

The problem thus presented to the subject was such that the primary effort required was that of vision. The skill necessary to synchronize the operation of the key with the time the two spots were in line was of secondary importance. The width of the commutator segment was one-twelfth of the circumference, and since the test-object was rotated at a speed of 15 r.p.m. the time available for a successful operation of the key was one-third

of a second. This time was proved, by experiment, to be ample and offered no difficulty to the subject.

The experiment was begun by placing the two spots in line and then starting the disc to rotating. At intervals of about thirteen seconds the signal operated and the subject pressed the key the next time the two spots were in line; the master spot was illuminated, the key was held down, and the experiment went on. This process was continued for thirty minutes and at all times the subject was required to give the most concentrated attention to the rotating spot in order to follow it. The method thus offered no opportunity for rest or even for the relaxation of attention without having the change in attention appear at once to the experimenter. It was found that thirty minutes of steadily watching the rotating spot was sufficient to produce a "fatiguing" of the eye that could be measured by methods available. The test, in the majority of the cases, left the eyes of the subject in a reddened and watery condition and with no doubt in the mind of the owner that fatigue was present.

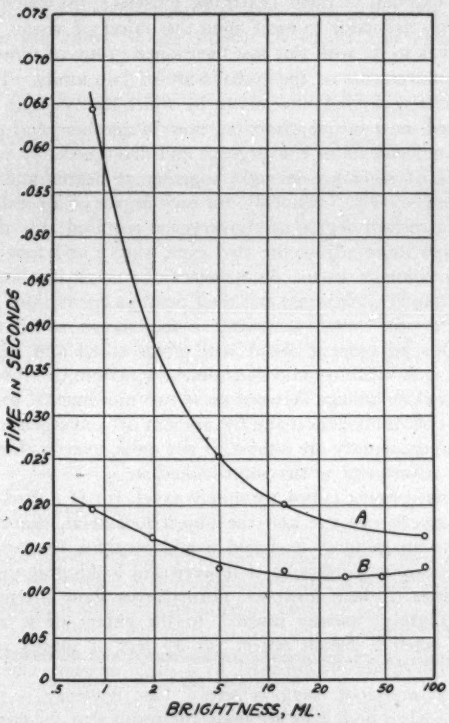
Since it was sought to measure the difference in eye fatigue, if any, as a function solely of the intensity of the illumination, the amount of work done under each condition must be the same. In the experiment this was accomplished by making the contrast and size of the test-object much more than would be required for threshold vision under any intensity used. Fig. 2 shows the relation between "brightness" and "speed of vision" for two different sizes of test-objects. While the data referred to are not exactly parallel to the problem of this test, yet the results from it can be applied reasonably well. Curves *A* and *B* show the relation between brightness and speed of vision for test-objects subtending 1.82 and 3.63 minutes at the subject's eye, and it is therefore obvious that with a test-object of 5.75 minutes as used in this experiment that a brightness of 5.0 ml. is much in excess of that needed to perceive the object. By a comparison of the speed at which the test-object in this experiment was operated against the "speed" of the test-object referred to by Fig. 2, it will be noted that the motion of the disc was comparatively slow.

With the arrangement described above no difference in the performance of the subject under the two intensities of illumination could be expected so far as the data taken from the counters is concerned. In order to have some means of determining the

relative fatigue of the eye in the two cases, the muscle balance of the subject was measured before and after the test and this data regarded as the criterion of " fatigue."

To show why muscle balance was selected as the measure of

FIG. 2.



Exposure time—brightness relations.

the effect of the half-hour's work upon the subject, it is first necessary to explain what is meant by muscle balance.

Certain muscular movements take place while the eyes are performing their adjustments and readjustments during active

use. The change in the pupil is one. The pupil diminished in diameter as the eyes readjust themselves from a far to a near object. Another adjustment consists in the accommodation (focussing) of each eye for the distance of the object. A third consists in the movements of the eyeballs brought about by the muscles external to them (extrinsic muscles), by which the two visual axes are made to meet upon the object of vision. Muscle balance has to do with this last-mentioned group of muscles.

The movements of the eyeballs are of two kinds. There are the associated parallel movements, by which the two eyes together are turned, now in one direction, now in another; and there are also the movements of convergence and divergence, by which the two lines of sight are brought together at nearer and remoter points, respectively. Obviously, for each degree of accommodation there is a certain degree of convergence required; for these two movements must adjust the two eyes, singly and together, for the same point in space. As a matter of fact, it has been found that the muscles concerned and their nervous connections with the central nervous system constitute a mechanism which normally brings this adjustment about with great speed and with great accuracy and certainty; and that somehow, within this mechanism, there is a close linkage between these two movements, so that the degree of accommodation and the amount of convergence brought about simultaneously are almost, if not quite, exactly those necessary for adjustment to the point looked at.

The adjustment is not absolutely exact, for if a card is interposed between one eye and the object looked at, the eye from which the image is so excluded would continue to stand in the same position as before, as if it were still looking at the object. As a matter of fact, however, normal eyes show a tendency to deviate slightly; usually inward, to the extent of a very few degrees, when a distant object (at 20 feet or more) is looked at; and almost always outward, by a slight amount, when a near object (at one foot, say) is seen. This tendency is promptly overcome when both eyes are used, the result that the two images are caused to fall upon corresponding parts of the two retinas. This is only a small part of the matter, as anyone will find by reference to a standard text-book on ophthalmology. It remains to state the considerations which led to the selection of this test as a measure of fatigue.

Fatigue, in the physiological sense, may be regarded as a temporary impairment of function following continued activity. The balance of the eye-muscles seemed to be a function of just such a highly coördinated and delicate mechanism as could be expected to undergo such impairment readily; and the mechanism is also one that is called into play to a great and perhaps trying extent in the effort of the subject to follow and identify the

Summary of Data.

Brightness, 100 ml.						Brightness, 5 ml.					
Muscle Balance.						Muscle Balance.					
Before Test.		After Test.		Arith. Dif.	Score.	Before Test.		After Test.		Arith. Dif.	Score.
▽	E	▽	E			▽	E	▽	E		
s 2.4	.115	s 2.7	.159	0.3	100	s 3.0	.193	s 3.6	.272	0.6	105
x 0.5	.038	s 0.1	.052	0.6	101	x 0.7	.053	s 0.2	.052	0.9	104
s 0.7	.060	s 0.9	.155	0.2	81	s 0.4	.059	s 1.2	.088	0.8	77
s 1.0	.041	s 0.7	.048	0.3	106	s 1.5	.014	s 1.1	.055	0.4	112
s 1.2	.072	s 0.3	.095	0.9	110	s 1.2	.072	x 0.1	.055	1.3	116
s 4.1	.138	s 2.9	.079	1.2	108	s 3.1	.126	s 3.1	.079	0.0	111
s 8.2	.167	s 6.6	.216	1.2	111	s 7.5	.173	s 6.3	.136	1.2	110
x 0.6	.038	s 0.3	.069	0.9	79	x 0.6	.069	s 0.6	.072	1.2	72
x 1.9	.079	x 1.5	.069	0.4	114	x 1.7	.045	x 1.4	.072	0.3	113
Average				0.67	101					0.73	102
Probable Error				0.105						0.108	

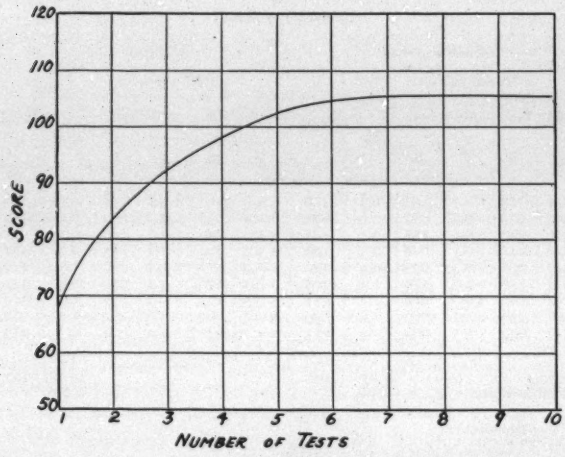
(x) Exophoria.
(s) Esophoria.
(▽) Centrad; equivalent to 1/100 of a radian.
(E) Individual probable error.

master spot among the twelve on the rotating disc. Moreover, the test is one recognized in ophthalmology, and it is fairly simple of application with small expenditure of time.

The table gives a summary of the results obtained from nine subjects taken on five occasions for each intensity of illumination. The quantities listed under the heading "score" are the number of correct operations performed out of 120 trials. The average scores for both intensities are practically identical and thus establish the fact that the work was done equally well under 5 ml. and 100 ml. (one millilambert, ml., is the brightness of a perfectly diffusing surface having an 80 per cent. reflection factor and an illumination of 1.16 foot-candles). In this average the score obtained the first and second tests (100 and 5 ml., respectively)

were eliminated, as these data were at great variance with following tests due to unfamiliarity of the subjects with the work. The 100 ml. and the 5 ml. were used on alternate tests. Fig. 3 gives the average "learning curve" for all subjects and includes all tests, ten in number, and indicates that after slight practice the subject has no difficulty in the manual operation of the key to

FIG. 3.



"Learning curve."

register the time when the master spot is opposite the selected stationary spot.

The subjects were fairly representative and showed various amounts of esophoria and exophoria before the experiment. There was obtained, however, a significant shift in the balance of the extrinsic muscles produced by the thirty minutes of continuous ocular effort. The point of balance changed during the test to the algebraic average amount of 0.67 and 0.73 centrads for 100 and 5 millilamberts, respectively. (One centrad equals one-hundredth of a radian.) For each value in the table, twenty-five individual readings from the optometer were averaged and the probable error computed. Since in both cases the change in the muscle balance position was some six times the probable

error, the shift must be regarded as significant and indicates that within the thirty minutes of test the muscle balance of the subject was altered to an appreciable and measurable degree.

Although the task to be performed by the eyes could be accomplished as well under the lower illumination, the *higher intensity was in no way harmful or tiring to the eyes of the worker.*

In the past many tests in the laboratory and in the industries have established the fact that more [2] and better work [3] can be done under conditions of proper lighting and from the results discussed in the foregoing paragraphs eye fatigue does not appear to detract in any way from the advantages of "high-intensity" illumination.

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[2] P. W. COBB: "Some Experiments on the Speed of Vision," *Trans. I.E.S.*, 19, 150.
[3] P. W. COBB and F. K. Moss: "The Effects of Brightness on the Precision of Visually Controlled Operations," JOUR. FRANK. INST., Apr., 1925.

Fixation of Nitrogen in France.—In an address before the Faculty of Applied Science of the University of Brussels, Georges Claude detailed at some length his work in connection with the perfection of a method for uniting hydrogen and nitrogen. The address was published in the *Bull. of the Indus. Federation of Belgium*, and republished in *Ann. Chim. App.*, etc. ([2], 1925, 7, 152), from which the following abstract is taken. Claude has been very prominent in developing the synthetic ammonia method, along lines similar to those that the *BASF* uses under Haber's guidance. The essential features of these are the employment of very high pressure, high temperature, and the association of a catalyst. The last is, indeed, the crux of the method, and so far the catalyst has not been definitely stated. In the address, Claude reviews the earlier steps of the method and especially the known details of the German procedure. He claims a much higher yield than the *BASF* obtains even now, by using very high pressures. He reviews the recent history of the Haber method, pointing out that it was not until 1905 that a definite synthesis of ammonia was effected, notwithstanding the fact that theory indicated such a result. In that year, Haber, operating at about a red heat, succeeded in obtaining a small yield of ammonia, by using a catalyst. The mechanical problem in using very high pressures with moderately high temperature is a serious one, as most metals suffer material loss of tensile strength when strongly heated. Claude asserts that the honor of having shown the method of constructing apparatus that

would bear a pressure of about 1000 pounds to the inch at a high temperature belongs to Nernst and not to Haber. The earlier experiments were, however, discouraging, Haber, himself, in a discussion with Nernst, at a meeting of the Bunsen Society, expressed fears as to the operation of a process involving such pressures, yet it was Haber who afterwards employed as high as 3000 pounds, having observed the catalytic powers of certain uranium compounds. In this way, Claude remarks with some pathos, a simple laboratory experiment became, under the support of a great chemical company, the means of prolonging for "three mortal years" a terrible war. The reference is to the fact that without this nitrogen fixation process, Germany would have been soon brought to a standstill for lack of nitric acid and fertilizers. It was stated in a recently published German work on nitrogen fixation that from the establishment of the blockade in August, 1914, until the middle of the next January only 5000 tons of sodium nitrate reached German ports. It seems likely that if the Haber process had not been installed, Germany would not have gone into the war.

Claude claims much superiority in yield in his method, which he designates as "hyperpressure," obtaining about 40 per cent. of the materials in the form of ammonia while the Haber method gives only about 6 per cent. Moreover, by passing the gas at the high pressure through pipes surrounded with cold water, the ammonia is obtained in liquid form and if the gases originally introduced are dry, anhydrous ammonia is obtained. He operates at about 13,500 pounds to the inch (900 atmospheres). The apparatus is not described in detail, and only a portion is figured, but a circulation of the gases in tubes is used and by treatment four successive times, as high as 87 per cent. yield is obtained. A special alloy is used for the tubes, and it is stated that some of these have already been operated for 7000 hours and show no signs of failure. Finally, it is stated that a factory is being erected in France which will have a capacity of 60 tons (presumably metric tons) of anhydrous ammonia per day.

H. L.

Biochemistry of Corn.—M. HELEN KEITH (*Univ. of Ill. Agr. Exp. Sta. Bull. No. 257*, 151 pages, 1925) has prepared an exhaustive bibliography of researches bearing on the composition and nutritive value of corn and corn products. One section is devoted to authors and gives the complete citation, including title in English, for each paper on corn published by each investigator. Another section is devoted to a bibliography by subjects, which are treated under two headings, purely chemical studies of corn and animal experiments. Among the subjects discussed under these headings are the composition of corn, its oil, proteins and starch, cob, pollen, spoilage, digestion, nutritive value, etc. The final section is devoted to selected books, pamphlets, and general articles.

J. S. H.

STIMULATION OF RESEARCH AND INVENTION.*

BY

D. S. JACOBUS, Dr. Eng.

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STIMULATION of research and invention is essential to progress and prosperity. There could be no more fitting an opportunity to say something on this subject than at the Centenary Anniversary of an institution that has been preëminent in encouraging scientific effort, and in connection with the inauguration of a Research Foundation established for the purpose of encouraging research. The Franklin Institute has accomplished much along the very lines for which the work of the Bartol Research Foundation will form a part, and is eminently fitted for handling this trust in a field in which it has already been successful.

Success in research usually comes from the individual. While this may not wholly apply in some cases of coöperative research, it holds in the great majority of cases. In this respect research is akin to invention. Progress in research can in most cases be best accomplished by the encouragement of the individual. It is in inspiring and encouraging the individual to put forth scientific effort that The Franklin Institute has played a most important part. It is, therefore, a happy coincidence that on the one hundredth anniversary of the birth of a society that has done so much in inspiring effort it is given an added opportunity through having funds at its disposal for doing good in its chosen field.

It is no easy matter to properly administer a research fund. There are two lines of usefulness, the promotion of research and the conduct of research. The coördination of effort to avoid duplication has been under discussion by the Engineering Foundation and by the Engineering Division of the National Research Council. Engineering Foundation is a trust fund for the furtherance of research in science and engineering established by Ambrose Swasey by gifts to the United Engineering Societies. The National Research Council is organized under a Congressional Charter of the National Academy of Sciences. These organiza-

* Address delivered Friday, September 19, 1924, on the occasion of the celebration of the centenary of the founding of The Franklin Institute.

tions coöperated during the war and much useful work was accomplished. While there cannot be an exact dividing line between the fields of work of organizations so closely akin and having mutual interests, it would seem that foundations such as the Engineering Foundation and the Bartol Foundation could do the most good by expending their available funds on the conduct of research, not, of course, by actually doing or directing the research work, but by providing means for assisting those who are actually doing or directing the work. It is a difficult task to accomplish this to the best advantage and no hard and fast rules can be laid down.

Research and invention go hand in hand. The question will come up of whether the party who is assisted by a foundation will be allowed to patent a development. While this question may not be of importance in connection with the work of the Bartol Foundation in view of its purely scientific character, it is important in connection with research in general and will be discussed. There is also the question of publicity and whether the results of work that is done shall be made available to the public, which in some cases might interfere with the patenting of an invention. These, with other troublesome questions, such as what cases should receive assistance, will always require careful investigation and mature judgment before arriving at a decision. There is many a striving professor who, should he be able to secure funds for conducting research work without signing off all right and title to the outcome of his efforts, would be willing to contribute his own time free of charge. The striving professor is only one of a type that should be helped.

To search out an individual of promise and to appropriate the proper amount of money for any particular research requires exceptional ability. The better qualified those who are handling a fund, the fewer the rules need be. Many are working on the problem of how a research fund should be administered and it would be presumptuous for any single individual to propose rules except for general discussion. It is in this spirit that the following are suggested.

(1) Encourage research of a scientific character which will be generally useful. Most researches of this kind have specific objects in view and do not lead to inventions except as an incidental result of some feature bearing on the research.

(2) Make no general restrictions respecting the patenting of inventions. Success in research comes through incentive as much as through initiative, and should an investigator be so fortunate as to develop something that is patentable and to secure a valuable patent, this will serve a most useful purpose in stimulating and encouraging others.

(3) Require periodic reports which will be held in confidence. Do not insist on publication in all cases. Insist that no matter be published except by the mutual consent of the party doing the research and of the foundation.

(4) Do not grant money for aimless research, but do not expect all researches to result in valuable contributions. Those who have strived to solve the secrets of nature know only too well that a new start must often be made and much matter discarded.

Applying the Golden Rule and treating the party conducting a research in the way those administering the fund would like to be treated will give the very best results. With a broad policy those having a fund in charge require the services of a true physicist having that power of perception which will enable him to correctly pick his men. This means the securing of an exceptional man. Such a man cannot be bought by offering a high salary, he must be sought and found, and if he has the right capabilities his value to the foundation will far exceed anything he may be paid. In any event care should be taken to conform with safe and sane business principles and to keep the expense of administering a fund in line with what it should be in proportion to the amount expended in actual research.

The most useful research work may not lead to invention, for example, Engineering Foundation and National Research Council coöperated with others in the work that was done by Prof. H. F. Moore at the Experiment Station at the University of Illinois on the fatigue phenomena of metals. This investigation led to results that are useful to engineers at large and represents a class of work that should surely be encouraged. Another research respecting which there can be no question as to whether it should receive encouragement is that which is being carried on under the auspices of The American Society of Mechanical Engineers to obtain a more exact steam table and one giving values for the higher pressures and temperatures. A number of industrial organizations have contributed toward the expense of making the tests and Engineering Foundation has endorsed the movement by making a contribution. In this investigation none of those in charge of the work receives any recompense, all of the money col-

lected being expended either in making the apparatus or in the actual conduct of the tests. Dr. Harvey N. Davis, of Harvard University, has supervised the making of a number of tests at the Jefferson Laboratory to determine the Joule-Thomson effect over a large field. Dr. Frederick G. Keys, at the Massachusetts Institute of Technology, is supervising the measurement of the pressure, temperature and volume figures in the saturated as well as the superheated region with a special apparatus of his design. Dr. N. S. Osborne, at the U. S. Bureau of Standards, is in charge of the determination of the thermal capacity of water as saturated liquid. A research foundation would be fortunate in securing work of this kind to foster.

Mention has been made of coöperation in research. It is only through coöperation that the best progress can be made in certain classes of research. This was exemplified during the war when some of the very best and most useful research work was done by the laboratories of large corporations. A great deal of research is done under the direction of corporations which have specific objects in view, and valuable as this is, it must be supplemented by individual research in order to have well-balanced scientific progress.

Where there is coöperation in research it is often hard to say who is the inventor in case of a development. The same applies to the work of any industrial organization. The party who first conceives a way or method of doing a thing and puts it into tangible form is technically the inventor, whereas had he not been working with others his mind might not have been directed along the particular channels that led to the invention. There are few valuable inventions that have been developed by parties working alone. When working with others most inventions come through the following up of a line of thought in which many have taken part. There is often a tendency for the inventor to overlook this feature, and feel that through being the one to patent an idea he should receive an undue amount of credit or compensation. The same applies to any successful issue in research work, whether it is patentable or not.

There should be a distinction between a patent on an improvement that was the object of a research and a patent that is an incidental result of some general research. Where a research worker is given a very definite problem of improving, for instance, some

existing thing, he should not be permitted to patent such an improvement with a view of deriving extraordinary benefit from something which he was definitely employed to do. It may be desirable to take out patents in a case of the sort in order to prevent designing people from securing patents for their own benefit on certain features of the work and thereby defeat the very object of the work. The publication of articles and reports, which are of necessity general in their presentation, cannot be depended upon to act as an anticipation in the same way that a patent does, and there is, therefore, considerable risk that once the general results of a research are known, designing people will endeavor to secure patents on some of the features. Where patents are taken out on an improvement of the sort a definite arrangement should exist with the research worker by which the patent can be made public either by dedication by the inventor or by the research foundations which provided the facilities.

Where the problem is a general investigation of a large mass of phenomena, or where the research worker is working on a problem of his own making, then, if in connection with such an investigation he makes an invention which may have come to his mind merely as an incident to his general work, he should have the right to patent the invention as his own. Even though this feature may be an embarrassing one which may call for a number of special rulings, I firmly believe that in general there should be no restriction put on the worker respecting the patenting of anything he may develop, and that any feature of an investigation that can be patented should be patented because, if this is not done, some unprincipled person may endeavor to take advantage of the opportunity to secure a patent of his own.

Research foundations may be made useful in avoiding as far as possible the duplication of research, but an attempt should not be made to go too far in directing research. To secure the best results an ambitious worker must have a free hand and it is this type of worker that should be given encouragement and assistance.

The Ejection of Protons from Nitrogen Nuclei, Photographed by the Wilson Method. P. M. S. BLACKETT. (*Proc. Royal Soc.*, A 742.)—Rutherford and Chadwick have shown that rapidly moving α-particles under certain conditions can eject protons from the nuclei of light elements. The author desires to learn how the three bodies

concerned, the nucleus, the proton and the particle, comport themselves after the disastrous collision. The scintillation method can detect the proton alone, but the others have too short a range to register their arrival by a flash of light on the screen. The cloud method of C. T. R. Wilson, however, can furnish information concerning all three bodies. There will be but few cases at best in which protons will be projected from nuclei, in fact, 1,000,000 α-particles of 8.6-cm. range will give rise to only about twenty such cases in nitrogen. Accordingly a Wilson apparatus was devised, making an expansion and taking a photograph of the tracks every ten or fifteen seconds. About 23,000 photographs were taken of tracks in nitrogen. "The average number of tracks on each photograph was 18; the tracks of about 270,000 α-particles of 8.6-cm. range and 145,000 of 5-cm. range have therefore been photographed." These numbers are rather appalling, yet among the vast number of collisions recorded only eight differed greatly from the rest. "These eight tracks undoubtedly represent the ejection of a proton from a nitrogen nucleus. It was to be expected that the photograph of such an event would show an alpha-ray track branching into three. The ejected proton, the residual nucleus from which it has been ejected, and the alpha-particle itself, might each have been expected to produce a track. These eight forks, however, branch only into two." The proton leaves a fine, straight track, while the second branch resembles the track of a nitrogen nucleus. There is no third branch. "On the generally accepted view, due to the work of Rutherford, the nucleus of an atom is so small, and thus the potential at its surface so large, that a positively charged particle that has once penetrated its structure (and almost certainly an alpha-particle that ejects a proton must do so) cannot escape without acquiring kinetic energy amply sufficient to produce a visible track. As no such track exists, the alpha-particle cannot escape. In ejecting a proton from a nitrogen nucleus the alpha-particle is therefore bound to the nitrogen nucleus." This conclusion is confirmed by quantitative study of the effects observed. G. F. S.

Notes on Neodymium Oxide. E. L. NICHOLS. (*Proc. Nat. Acad. Sciences,* Jan., 1925.)—Neodymium oxide is interesting because it gives a discontinuous spectrum though it is a solid. The light from it, provided its temperature is not much above 1000° C., shows six bands. "At higher stages of incandescence the bands merge and the spectrum cannot be distinguished from that of a black body of like temperature." In order to get the band spectrum a hydrogen flame must impinge on the oxide. Another peculiarity of the substance is that at about 900° it is brighter than a black body of the same temperature. "At the above temperature, therefore, there is luminescence of the oxide superimposed upon the incandescence." G. F. S.

NOTES FROM THE U. S. BUREAU OF CHEMISTRY.*

TOLUIDINE DERIVATIVES. I. QUANTITATIVE PREPARATION OF 5-IODO-2-AMINOTOLUENE AND SOME OF ITS DERIVATIVES.[1]

By Raymond M. Hann and Julius F. T. Berliner.

[ABSTRACT.]

A QUANTITATIVE method for the preparation of 5-iodo-2-aminotoluene (5-iodo-o-toluidine) has been devised in the Bureau of Chemistry.

The following derivatives of 5-iodo-o-toluidine were prepared and analyzed: Hydrochloride, hydrobromide, hydrofluoride, hydriodide, perchlorate, picrate, picrolonate, mercurichloride, oxalate, urea, urea hydrochloride, phenylurea, α-naphthylurea, and isocyanide.

THE ADDITION COMPOUNDS 3, 5-DIBROMO-O-TOLUIDINE WITH METALLIC SALTS.[2]

By Raymond M. Hann and G. C. Spencer.

[ABSTRACT.]

DURING a comparative study of two methods for the determination of nitrogen, it became a matter of interest to test the effect of metals upon the catalytic oxidation of organic matter by sulphuric acid previous to final conversion of all nitrogen to ammonium sulphate. The addition compounds of anilines with metallic halides seemed suitable for this study, as they contained nitrogen and the desired metal in a single compound. Several such compounds were made.

It was found that 3, 5-dibromo-o-toluidine combines with metallic halides to give addition compounds containing two molecules of base to one molecule of metallic salt.

* Communicated by the Chief of the Bureau.

[1] Published in *J. Am. Chem. Soc.*, 47 (June, 1925): 1709.

[2] Published in *J. Wash. Acad. Sci.*, 15 (April 19, 1925): 163.

The mercuric chloride, zinc chloride, cadmium bromide, and cadmium chloride 2:1 addition compounds were prepared and analyzed.

The presence of the metals did not adversely affect the nitrogen results as determined by the official method.

A COMPARATIVE STUDY OF THE GUNNING-ARNOLD AND WINKLER BORIC ACID MODIFICATIONS OF THE KJELDAHL METHOD FOR THE DETERMINATION OF NITROGEN.[3]

By K. S. Markley and Raymond M. Hann.

[ABSTRACT.]

EXTENSIVE tests have shown that the boric acid absorption method is as accurate as the Kjeldahl-Gunning-Arnold official method, provided water-cooled condensers are used and care is taken that the temperature in the receiving flask does not exceed 50° C.

Its advantages over the official method are:

(1) The constant use of a standard alkali, with the attendant checking necessary because of a changing alkalinity, is eliminated.

(2) Careful measurement of the fixing medium is not necessary. If for any reason the fixing medium is sucked back into the distilling flask, no damage is done. More boric acid can be added to the receiving flask and the distillation continued.

(3) Bromphenol blue may be used in artificial light, thus permitting titration to the same end point, irrespective of varying light conditions in the laboratory.

A comparison of the efficiency of mercuric oxide-potassium sulphate with that of copper-phosphoric anhydride has shown that both combinations give satisfactory results with a large number of nitrogenous compounds. With certain nitrogenous compounds, especially alkaloids, copper sulphate-phosphoric anhydride combination necessitates longer digestion; in other cases it acts as rapidly as the mercuric oxide-potassium sulphate. The use of the copper sulphate offers the advantage that no catalyst need be made inoperative before distillation.

[3] Published in *J. Asso. Official Agr. Chemists*, **8** (May 15, 1925): 455.

PSILOCYBE AS A FERMENTING AGENT IN ORGANIC DÉBRIS.[4]

By Charles Thom and Elbert C. Lathrop.

[ABSTRACT.]

A STUDY by the Bureau of Chemistry of the fermentations occurring in large masses of bagasse led to the isolation of a species of Psilocybe capable of producing fermentation in this product. Mycelia were found to penetrate baled bagasse where common moulds were unable to develop. This development was extremely rapid and was accompanied by easily determinable visual changes in the fibrous mass.

The great activity shown by this Psilocybe in pure culture and in the field, considered together with the abundance of such forms in connection with decaying plant remains, suggests the need of intensive study of the habits and possibilities of the mushrooms as agents of decomposition, especially in the soil.

BACTERIAL CONTENT OF SWEETCORN.[5]

By Lawrence H. James.

[ABSTRACT.]

SWEETCORN, selected as representative of the type of vegetables usually cooked before consumption, was studied in the commercial cannery, as most sweetcorn reaches the consumer through this agency.

Examination of ten ears of fresh corn showed an average of 30,000 organisms per kernel growing at 30° C. and one thermophile.

The bacterial count increased from 135,000 organisms on hand-husked corn to 22,500,000 per kernel in corn passing through the sifting screens.

Before being mixed with the corn, the brine contained twenty-two thermophiles per c.c. After mixing and preheating to 185° F., more than 99 per cent. of the 30° organisms and more than 90 per cent. of the heat-resistant organisms were killed.

Corn packed in an insulated barrel, to simulate the centre of a large corn pile, showed the following progressive changes during a four-day experiment: (1) Increase in temperature to 51° C.;

[4] Published in *J. Agr. Res.*, **30** (April 1, 1925): 625.

[5] Published in *Canning Age*, **6** (June, 1925).

(2) large increase in 30° organisms at the end of the first twenty-four hours and marked decrease at end of first forty-eight hours; (3) gradual increase in thermophiles after first forty-eight hours; (4) gradual softening of corn and growth of mould, until at the end of the fourth day the corn was very mouldy, wet, and brown; (5) increase in acidity.

Corn which had remained in a pile over night increased in temperature to 50° C., and in bacterial count to 580,000 organisms per kernel. When left in a wagon over night, wet corn was hot and steaming, with a temperature of 55° C., and a bacterial count of 10,000,000 organisms per kernel by the following morning. Kernels were split and browned from the heat.

THE ODOROUS CONSTITUENTS OF THE COTTON PLANT. EMANATION OF AMMONIA AND TRIMETHYLAMINE FROM THE LIVING PLANT.[6]

By Frederick B. Power and Victor K. Chesnut.

[ABSTRACT.]

AN EXTENSIVE study was made in the Bureau of Chemistry to determine the chemical character of the odorous constituents of the cotton plant.

The essential oil, as obtained by extracting a concentrated distillate of the cotton plant with ether, had the following characters. It was a pale brownish-yellow, limpid liquid, having a strong, rather agreeable and persistent odor, $d_{25}^{25} = 1.4797$; $(\alpha)_D^{20°} = -3.91°$. It was found to contain an aldehyde and give a strong reaction for furfural.

The concentrated distillate from the plant, which represented all its odorous and volatile constituents, was found to contain the following individual substances:

(1) Methyl alcohol, in large amount, and traces of acetone.

(2) Amyl alcohol, in relatively small amount, with small amounts of higher homologs.

(3) Acetaldehyde, and traces of an aldehyde of higher carbon content. A crystalline condensation product of acetaldehyde with *m*-nitrobenzhydrazide has been formed; m. p., 151–152°. This is apparently a new compound.

(4) Vanillin, $C_8H_8O_3$, in very small amount.

[6] Published in *J. Am. Chem. Soc.*, 47 (June, 1925): 1751.

(5) A phenol, in exceedingly minute amount. This substance is either a derivative of *m*-cresol or a phenol that possesses very similar characters.

(6) An optically inactive dicyclic sesquiterpene, $C_{15}H_{24}$.

(7) A new, optically active tricyclic sesquiterpene, $C_{15}H_{24}$.

(8) A small quantity of a paraffin hydrocarbon, m. 62°, which apparently is triacontane, $C_{30}H_{62}$.

(9) A blue oil, which probably contains the highly unsaturated hydrocarbon, azulene, $C_{15}H_{18}$.

(10) Formic, acetic and caproic acids, the last in small proportion, which evidently were present to some extent in combination with the previously mentioned alcohols as esters.

(11) Ammonia.

(12) Trimethylamine.

Ammonia and trimethylamine were present in appreciable quantities in the distillate, but the ammonia largely predominated. Both ammonia and trimethylamine were also found to be emanations from the living plant.

Although trimethylamine has been found by field tests to have some attraction for the boll weevil, much more extended experiments must be undertaken by the entomologists in order to determine the practicability of its use as a bait.

Ether-drift Experiments at Mount Wilson. DAYTON C. MILLER. (*Proc. Nat. Acad. Sciences,* June, 1925.)—In Cleveland in 1887 Michelson and Morley performed their famous experiment, the purpose of which was to determine the relative motion of the earth and the luminiferous ether. "The experiment is based upon the argument that the apparent velocity of light should be slightly different according to whether the observer is carried by the earth in the line in which the light is travelling, or at right angles to this line." The essential feature of the experiment lay in the observation of the position of fringes produced by the interference of two rays of light that had traversed paths at right angles to each other. The two collaborators stated their conclusion thus: "Considering the motion of the earth in its orbit only, the observations show that the relative motion of the earth and the ether is probably less than one-sixth the earth's orbital velocity and certainly less than one-fourth." The relative motion was therefore according to them less than 7½ km. per sec.

Lord Kelvin felt that this fundamental experiment should be repeated with more sensitive apparatus. In consequence Morley and Miller constructed an interferometer about four times as sensitive

as its predecessor and with it made observations in the basement of Case School of Applied Science in Cleveland in 1904 and 1905. They summed up their results thus: "We may therefore declare that the experiment shows that if the ether near the apparatus did not move with it, the difference in velocity was less than 3.5 kilometres per second, unless the effect on the materials annulled the effect sought. Some have thought that this experiment only proves that the ether in a certain basement-room is carried along with it. We desire, therefore, to place the apparatus on a hill to see if an effect can be there detected." This they did, but before definite results were assured, the attempt was brought to an abrupt end by the demand of the owner of the site for the removal of the interferometer. Not until 1921 was the experiment resumed and in the meantime Professor Morley had died. Professor Miller in that year installed his apparatus on Mount Wilson, and from April 9th to April 21st made 1300 observations of ether drift. From these he was able to calculate the relative motion of the earth and ether in km. per sec. and further to specify the line in which the drift took place. Many alterations in the details of manipulation were tried with no effect upon the results. The apparatus was taken back to Cleveland and there used from 1922 to 1924. The displacement of the fringes beside Lake Erie was less than on the Californian mountain. In 1924 again the apparatus was set up on Mount Wilson, but in a location chosen to obviate a certain possible source of disturbance, but substantially the same results appeared. In April, 1600 additional observations were made. A curve based on these shows how the ether drift changes in magnitude and direction with the time of day.

"The ether-drift experiments at Mount Wilson during the last four years, 1921 to 1925, lead to the conclusion that there is a relative motion of the earth and the ether, at this observatory, of approximately nine kilometres per second, being about one-third of the orbital velocity of the earth. By comparison with the earlier Cleveland observations, this suggests a partial drag of the ether by the earth, which decreases with altitude. A complete calculation of the observations, now in progress, together with further experiments in the immediate future, should give definite indications regarding the absolute motion of the solar system in space." G. F. S.

Chemical Products of Florida.—WALTER H. BEISLER, of the University of Florida (*Jour. Chem. Education*, 1925, **2**, 437–439), states that the chief chemical products of Florida are phosphate rock, wood products, tung oil, sand and lime for the glass industry, kaolin, fuller's earth, and rare minerals. Florida produces approximately 67 per cent. of the phosphate rock and 75 per cent. of the fuller's earth mined in United States. The wood products include turpentine, rosin, and charcoal and the by-products of the destructive distillation of wood. Tung oil is a drying oil. The rare minerals include ores of titanium, zirconium, thorium, and cerium. J. S. H.

NOTES FROM THE U. S. BUREAU OF MINES, DEPARTMENT OF COMMERCE.*

TREATMENT OF MANGANESE SILVER ORES.

By Galen H. Clevenger and Martinus H. Caron.

In coöperation with the Netherlands East Indies Government, the Bureau of Mines has developed the Caron process for treating manganese-silver ores, in connection with the Clevenger roasting furnace. Although there are exceptions, oxidized silver ores containing the higher oxides of manganese are generally refractory to hydrometallurgical methods of treatment. When these ores are of high enough grade they can be smelted; indeed, some rather low-grade ores of this type have been smelted at a profit because of their fluxing value. In the past, when these ores could not be smelted, they either were not treated or were treated at low efficiency, generally by cyanidation. The need for thorough investigation of the problem was therefore evident for some time before the writers began their research.

The investigation proved that there is a refractory compound of manganese and silver—probably a manganite—insoluble in cyanide solution and other common solvents for silver. Although this compound has not been isolated from natural ores, it has been made synthetically, and shows all the characteristics of the natural product. Furthermore, the authors have shown definitely that heating in air a relatively small amount of a silver compound with a large excess of silica (in the proportions ordinarily occurring in a commercial silver ore) renders increasing percentages of the silver, depending on the temperature, insoluble in cyanide solution. Whether this is a physical or a chemical effect may be debatable, but the evidence the writers can offer seems to favor the formation of a chemical compound, such as silver silicate. The behavior of silver with silica has an important bearing on the problem.

The Caron process is based on the discovery that when oxidized ores containing a refractory compound of silver and manganese are heated in a reducing atmosphere—so as to reduce completely the higher oxides of manganese to manganous oxide—and are cooled under conditions that will prevent reoxidation,

* Communicated by the Director.

the refractory compound is decomposed and the silver is rendered amenable to cyanidation. The Clevenger furnace and cooler unit has been developed, whereby reduction can be obtained with high fuel economy and reversion to the refractory state can be prevented. Fuller details are given in Bulletin 226 of the Bureau of Mines.

TOXIC GASES FROM HIGH-SULPHUR PETROLEUM.

By R. R. Sayers, N. A. C. Smith and others.

A STUDY of the toxic gases from certain high-sulphur crude oils from Mexican and other high-sulphur petroleums and products was conducted by the Bureau of Mines and the Public Health Service in coöperation with the American Petroleum Institute. The work was divided into two parts, namely, field work and laboratory investigations. The most interesting and important of the laboratory findings were: (1) The presence of hydrogen sulphide (H_2S) in the gases and vapors from light Mexican crude; (2) the extreme toxicity of H_2S; (3) the absence of arsine, stibine, phosgene, hydrocyanic acid, and other toxic substances; (4) treatment recommended for poisoning by H_2S; (5) use and limitations of oxygen breathing apparatus, hose masks, and gas masks.

The investigations in the field were made at refineries handling Mexican crude, at some of which accidents from gas had occurred. The difficulties encountered, the means used to combat this hazard, the gas accidents that had occurred, and the conditions existing where the accidents took place were studied.

In general, the field investigations confirmed the findings of the laboratory investigations. Hydrogen sulphide was found in storage tanks, stills, gas pipe lines, and other equipment used in refining high-sulphur crudes. Its toxicity was amply demonstrated by plant records of accidents caused by the gas. The most hazardous places at refineries were determined, and the precautions and safety methods developed by the companies, and the methods of emergency and after-treatment of cases were carefully studied.

In all gas accidents studied the men affected were close to the apparatus from which the gases escaped. The gases are rapidly diluted in air and, consequently, they would not be harmful

a short distance away. No cases of poisoning, except in a refinery or at points closely adjacent to storage tanks or places where high-sulphur oils are handled, have been called to the attention of the authors. This work is more fully described in Bulletin 231 of the Bureau of Mines.

TESTS OF A LARGE BOILER FIRED WITH POWDERED COAL.

By Henry Kreisinger, John Blizard, C. E. Augustine, and B. J. Cross.

Twenty-six tests of a four-pass Edgemoor boiler, fired with powdered coal at the Lakeside station of the Milwaukee Electric Railway and Light Company, were made by the fuel section of the Bureau of Mines in Coöperation with the research department of the Combustion Engineering Corporation and the operating department of the Milwaukee Electric Railway and Light Company. The object of the tests was to determine the thermal efficiencies and capacities obtainable by burning powdered coal under large central-station boilers, and the possibility of operating such boilers continuously at high efficiency and capacity without destructive effect on the furnaces and without difficulties in refuse removal.

In any system for burning pulverized coal the furnace is the most important part because, to justify the cost of pulverizing, the powdered coal must be burned with higher efficiency than coal fired with mechanical stokers.

For high thermal efficiency the coal must be burned almost completely with low excess air. But low excess air and complete combustion give high furnace temperature, which in turn causes erosion of the furnace lining and fusion of the ash; and removal of the fused ash is difficult. The necessity of complete combustion with low excess air thus brings with it these two important problems in the design of a furnace for burning powdered coal: (1) Prevention of erosion of the furnace lining; (2) easy removal of refuse from the furnace.

For the purpose of solving these problems, the boiler tested was equipped with a furnace having hollow walls, through which about 60 per cent. of the air needed for combustion passed before it entered the furnace. By passing through the hollow walls, the air cooled the furnace lining and prevented excessive erosion. A water screen was placed in the furnace about three feet above

the bottom to keep the refuse deposited at the bottom from fusing and to make its removal easy.

The coal used in the first five tests came from Illinois. The fuel in the other tests was a mixture of Illinois and Pittsburgh coals.

The rating at which the tests were made ranged from 130 to 257 per cent. of the rated capacity of the boiler. Ratings above 257 per cent. could not be obtained on account of insufficient draft. The exhaust fan on the economizer was designed for a maximum of 250 per cent. of rating, and could not be operated continuously above this rate of working.

The efficiency of the boiler and superheater ranged from about 86 to 80 per cent., and averaged about 84 per cent., which is about 6 per cent. higher than can be obtained with stoker-fired furnaces under similar conditions of careful operation. This high efficiency is due to the obtaining of nearly complete combustion with low excess air. The incomplete combustion losses were less than 2 per cent., and the excess air ranged from 5 to 30 per cent.

That the combustion could be made almost complete with small excess air is to be attributed to the ample size and suitable shape of the furnace, to the long path of the flame, and to the means for admitting and regulating the flow of air and coal.

The water screen, by partly screening the bottom of the furnace from the heat radiated by the flame, kept the ash deposit from fusing and simplified its removal. The hollow furnace walls proved a desirable feature, because the air passing through them cooled the furnace lining and greatly reduced the eroding effect of the molten ash. The tests are described in more detail in Bulletin 237 of the Bureau of Mines.

How to Get High Speeds of Rotation. E. Henriot and E. Huguenard. (*Comptes Rendus*, May 11, 1925.)—In the endeavor to reach high speeds of rotation, friction and the difficulty of getting perfect centring generally intervene to veto success. These investigators have devised a motor operated by compressed gas in which the rotating part is supported upon the escaping gas. This avoids friction and at the same time makes it possible for the rotor to select its axis of rotation for itself. Such a rotor, 11.7 mm. in diameter, was maintained for hours at a speed of 4000 r. per sec. and could be speeded up to 11,000 revolutions per sec. G. F. S.

THE FRANKLIN INSTITUTE.

MEMBERSHIP NOTES.

CHANGES OF ADDRESS.

MR. ROBERT J. ANDERSON, 221 Amber Street, E. E., Pittsburgh, Penna.
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NECROLOGY.

Mr. Jonathan R. Jones, 1800 Widener Building, Philadelphia, Penna.

LIBRARY NOTES.

RECENT ADDITIONS.

Architectura Hydraulica oder die Kunst das Gewaesser zu den verschiedentlichen Nothwendigkeiten des menschlichen Lebens zu leiten, in die Hoehe zu bringen und vortheilhafftig anzuwenden. Aufs grundlichste abgehandelt von Monsieur Belidor. Three volumes, folio. Augsburg, 1740–1744. Given by Mrs. Coleman Sellers, Jr.

American Concrete Institute.—Proceedings of the Twenty-first Convention. Volume 21. 1925.

American Engineering Council.—Industrial Coal: Purchase, Delivery and Storage: A Report. 1924.

American Gas Association.—Proceedings of the Sixth Annual Convention. 1924.

Beilstein's Handbuch der Organischen Chemie. Fourth edition, Volume 7. 1925.

CHRISTIANSEN, WALTER G.—Organic Derivatives of Antimony. 1925.

Engineering Index, 1924. 1925.

GALBRUN, HENRI.—Assurances sur la Vie: Calcul des Primes. 1924.

GEERLIGS, H. C. PRINSEN.—Cane Sugar and Its Manufacture. Second edition. 1924.

GUYE, C. E.—Physico-chemical Evolution. Translated by J. R. Clarke. No date.

Illustrated Technical Dictionary in Six Languages—English, German, Russian, French, Italian, Spanish. Volume 15. No date.

Index Generalis, 1924–1925.

Klimsch's Jahrbuch. Volumes 2–8, 11, 16, 18, 1901–1925. Ten volumes. No date.

MONTEL, PAUL.—Statique et Résistance des Matériaux. 1924.

NEWELL, LYMAN C.—College Chemistry. 1925.

OLIVIER, CHARLES P.—Meteors. 1925.

Photograms of the Year 1924.

Rathke's Jahr- und Adressbuch der Zuckerindustrie. Volumes 32–36, 38–39, 1915–1923. Seven volumes. No date.

Royal Society of London.—Catalogue of Scientific Papers. Volume 19. 1925.

SERVICE, J. H., and FREASE, G. E.—A Laboratory Manual of Machine Shop Practice. 1924.

ZIPPRICH, ANTHONY E.—Freehand Drafting. 1924.

BOOK REVIEWS.

INFLUENCE DU SYSTÈME DE TRIANGULATION SUR LES EFFORTS SECONDAIRES. Par Z. Bazant, Professeur à l'École Polytechnique de Prague. 50 pages, 6″ × 9″, paper, with 20 figures in the text and on 3 plates. Prague, Libraire Fr. Rivnác, 1923.

It is usual practice in the design of framed structures to determine the stresses by considering the joints pin-connected and free to adjust themselves to load-deformations without the production of bending stresses. Such a procedure is obviously sound for structures like the pin-connected railway bridges formerly much used in this country. Although pin-connections in railway bridges have been supplanted by rigid riveted joints, the determination of stresses on the hinge-connection basis is still current practice, and provision for the additional stresses induced by the resistance of the rigid joints to deformation is made in the specification of allowable unit stresses.

The desirability of determining secondary stresses by rational methods has long since been recognized and in recent years has received much study and has become to a certain extent codified, notably by the American Railway Engineering Association, but the computations are long and tedious and add materially to engineering costs.

The present essay is a notable contribution towards the evolution of a rational method of determining secondary stresses applicable to the requirements of the engineering office. The method employed consists in con-

sidering the truss as a continuous beam with supports at the panel-points which have been lowered a distance equal to their vertical deflections. By means of the theorem of three moments the bending moments at these points resulting from these unequal deflections are determined. From these moments the stresses induced are readily computed from the properties of the section. The influence of the web-members is ignored as negligible for the reason that their effect diminishes very rapidly with the distance from the support. The process of application and the steps in the derivation of the formulas employed are given in minute detail.

Six typical trusses are analyzed. For ready comparison these are of similar span and loading. The computations and tabular results are also given in the fullest detail.

A comparison of the results shows that the disposition of the web-members has an important influence upon the magnitude of the secondary stresses. In systems with semi-diagonals or semi-verticals derived either from the simple Pratt form of truss or from the Warren type, the secondary stresses in the first are increased about five times the corresponding values for the simple form of Pratt truss, and in the second about ten times. The advantages of the K-truss, which is among those analyzed, are pointed out. The secondary stresses in this form are but 2 per cent. to 4 per cent. of the primary stresses. How great secondary stresses may become with unfavorable forms of trusses is shown in the analysis of a double-intersection Pratt type of truss. There, the secondary stresses reach the value of 86 per cent. of the primary stress.

This work, for which its author has been awarded the scientific literature prize of the Masarykova Akademie Práce, should prove of deep interest to structural engineers. L. E. P.

ANALYTIC GEOMETRY AND CALCULUS. By Bolling H. Crenshaw, M.E., Head of Department of Mathematics, Alabama Polytechnic Institute, and Cincinnatus D. Killebrew, M.S., Professor of Mathematics, Alabama Polytechnic Institute. x–222 pages, 6″ × 9″, cloth. Philadelphia, P. Blakiston's Son and Company, 1925. Price, $2.75.

The extent of the mathematical knowledge which the engineering student may profitably acquire is in a great measure governed by his personal inclination, but there is an irreducible minimum which is essential to the prosecution of this course of study. Much experimenting has been done in recent times by teachers in technical schools with the view to developing a course covering the necessary and sufficient matter for the study of engineering subjects which are developed upon a mathematical framework. This movement has resulted in some instances in abandoning the time-honored formal treatises on analytical geometry and calculus for combined works in which these subjects are treated together. Such an arrangement yields the double advantage that the differential coefficient may be applied in dealing with tangents and normals, but what is still more useful, the student learns early in the course the process of differentiation and is thus enabled to proceed with the study of mechanical subjects.

The present volume is a work of this sort prepared to meet the particular needs of the authors' classes. It includes a brief but well-detailed account of the conic sections, but in which all reference to poles and polars is omitted.

The general equation of the second degree, free of the xy-term, is treated at considerable length. The criteria for determining the character of the curve are decidedly simplified with the equations in this form. However, since transformations must be made when the xy-term is present, this departure from the customary treatment does not seem altogether fortunate. The calculus is based on the usual method of limits and developed along conventional lines with exceptions here and there in the usual order of presentation.

As is desirable in any work and especially in a text-book, explanations are given fully, and numerous illustrative problems and adequate drill exercises add materially to its teaching value. It is safe to say that the student who has mastered the contents of this book will be well equipped to deal with the generality of engineering problems and well prepared for a study of various amplifications of these subjects which his professional welfare may prompt him to undertake.

LUCIEN E. PICOLET.

ORGANIC DERIVATIVES OF ANTIMONY. By Walter G. Christiansen, Harvard Medical School. American Chemical Society Monograph Series. 230 pages, 8vo. New York, The Chemical Catalog Company, 1925. Price, $3 net.

Antimony has had a long and somewhat peculiar history. The fable of the origin of name has no basis in fact, but is frequently told by lecturers. Antimony sulphide seems to be the substance to which originally was given the name that now has become our familiar "alcohol" with its social, legal and industrial importance. Basil Valentine's enthusiastic advocacy of antimonials as cure-alls was a striking feature of the development of chemistry. In contrast with it was the antagonism to all medical use of antimony compounds about the time of the American Civil War. A southern physician of note and experience declared against all use of these, and for a time official prohibition of their use extended to the northern army medical staff. At Jefferson Medical College in Philadelphia in the 60's of the last century, the professor of practice invariably advised against the use of tartar emetic. In its general chemical relations, antimony stands midway between the obvious metals and the obvious non-metals. Its physical appearance and its basic oxides and sulphides line it up with the metals, but its ready formation of a hydrogen compound brings it into close relation with nitrogen, phosphorus and arsenic. With the last named its affiliations are strong, shown both by analogy of compounds and analytic similarity.

The book in hand will be quite an interesting revelation to the general run of chemists, for the data show how extensive is the range of antimonial affinity in connection with organic residues. Research has been busy in studying the element in all its possibilities of forming organo-metallic compounds, and rich reward has followed the efforts. A powerful influence in promoting this research has been the value of such compounds in the treatment of the parasitic diseases of the tropics that have of late assumed such great economic importance. Doctor Shattuck contributes an interesting chapter on the therapeutic value of antimonials, discussing at some length the problem of attacking the parasites causing the several forms of true sleeping sickness. These diseases, which in some districts, especially in Africa, render impossible the use of ordinary draft animals, are usually carried by blood-sucking flies, commonly known, in

the reduplicated words of the native tribes, as "tsetse." These have long been known to the residents and explorers of tropical Africa, and lately through the cross-word puzzle the term has become generally familiar. The "sleeping sickness" of which so much is heard from tropical regions has no analogy to the sleeping sickness mentioned occasionally in reports from temperate regions. The parasitic organism is known as a trypanosome, of which there are several species. They are animal organisms, not bacteria. Some species seem to be innocuous to man, but others are most dangerous. There are also distinctions in the virulence of certain species to certain animals. In some instances the parasite is introduced by direct contact and not through an insect bite. Some species yield fairly well to antimonial organic compounds, but others are resistant. It appears, also, that treatment of an infected animal, if a complete removal of the parasite is not attained, may leave a strain more resistant than usual. This is probably an instance of "natural selection," a means of evolution which has been much under anathema of late years. A similar instance has been noticed in the boll-weevil. As this insect spread northward, though originally a warm-area creature, strains capable of bearing the lower temperature have appeared. "Eppur si muovo."

The book is a valuable addition to the literature of organic chemistry in its practical aspects, and shows how extended research may go on without attracting attention except from specialists. The reviewer wishes to suggest that expense will be materially diminished, and appearance improved if the committee in charge of these monographs will have a matrix cut so that the benzene hexagon can be cast as a solid type, thus avoiding "building-up" the cyclic diagram. The structural formulas in the present case are large and clumsy. In publishing the English translation many years ago of Weyl's "Sanitary Relations of the Coal-tar Colors," the present writer had such a matrix made and much economy of composition resulted with greater neatness of appearance. The naphthalene ring is easily made by shaving off one side of one of the benzene types.

HENRY LEFFMANN.

RADIO INSTRUMENTS AND MEASUREMENTS. Circular of the Bureau of Standards, No. 74. Second edition, paper, 345 pages, illustrations, 7″ × 10″. First edition prepared by J. H. Dellinger, J. M. Miller and F. W. Grover, assisted by G. C. Southworth and other members of the radio laboratory. Revision for second edition made by J. H. Dellinger, L. E. Whittemore and R. S. Ould. Washington, D. C., Government Printing Office, 1924. Price, sixty cents. Sold only by the Superintendent of Documents, Government Printing Office, Washington, D. C.

Among the many activities of the Bureau of Standards, not the least has been its investigations in radio communication, as its fine laboratory devoted to that art and the many monographs on the subject which it has issued bear witness. As the authors point out, radio communication has undergone frequent and radical changes, consequently methods and instruments of measurements peculiar to the radio art have not reached the degree of standardization attained in other fields of electrical science. The circular fills a distinct gap in the literature of the art in presenting in didactic form an account of the more important instruments and measurements actually used in radio work. A

familiarity with elementary electrical theory is assumed, but an excellent *résumé* which precedes the development of the subject provides the reader with a valuable introductory review which, while thoroughly modern, is in conformity with current practice.

The work is in three divisions with an appendix. In Part I, Theoretical Basis of Radio Measurements, are discussed the fundamentals of electromagnetism, the principles of alternating currents, radio circuits and damping. Part II, Instruments and Methods of Radio Measurements, contains a description and analysis of wave meters, condensers, coils, current measurement, resistance measurement and sources of high-frequency current. Part III, Formulas and Data, deals with the calculation of capacity, calculation of inductance, design of inductance coils, high-frequency resistance and miscellaneous formulas and data. The appendix, in three parts, contains an account of the radio work of the bureau, a bibliography and a list of the symbols used.

Those who are acquainted with the high order of merit of the publications of the Bureau of Standards will not be disappointed in this timely work on the quantitative features of a subject of widespread interest. L. E. P.

A LABORATORY MANUAL OF MACHINE SHOP PRACTICE. By Jerry H. Service, B. S. in E. E., Dean of Trade and Engineering Schools, the Youngstown Institute of Technology, and George E. Frease, Instructor of Shop Practice, the Youngstown Institute of Technology. xii–106 pages, illustrations, 5″ × 8″, cloth. New York, D. Van Nostrand Company, 1924. Price, $1.25 net.

As indicated by the title, this is a laboratory manual of machine shop exercises which are designed to illustrate the application of various fundamental operations. Ample directions are given for carrying out each exercise with numerous helpful suggestions as to the best mode of procedure. The exercises, which are illustrated, cover the essential operations of bench work, the drill press, engine lathe, milling machine and planer and shaper.

The absence of any reference to the use of the surface-plate and scraping to fit is a somewhat notable omission. Doubtless the authors have good reasons for not including an exercise in this essential to an accurate sliding fit. A book of this kind should prove of value in the shop work of an engineering course as well as in vocational training. L. E. P.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS. Report No. 208, Determination of Turning Characteristics of an Airship by Means of a Camera Obscura. By J. W. Crowley, Jr., and R. G. Freeman. 8 pages, illustrations, quarto. Washington, Government Printing Office, 1925. Price, ten cents.

This investigation was carried out by the Committee at Langley Field for the purpose of determining the adaptability of the camera obscura to the securing of turning characteristics of airships, and also of obtaining some of those characteristics of the C-7 air ship. The method consisted in flying the airship in circling flight over a camera obscura and photographing it at known time intervals. The results show that the method used is highly satisfactory and that for the particular manœuver employed the turning diameter is 1240 feet,

corresponding to a turning coefficient of 6.4, and that the position of zero angle of yaw is at the nose of the airship.

Report No. 212, Stability Equations for Airship Hulls. By A. F. Zahm. 5 pages, illustrations, quarto. Washington, Government Printing Office, 1925. Price, five cents.

The report includes simple formulæ for determining, directly from the data of wind-tunnel tests of a model of an airship hull, what shall be the approximate character of oscillation, in pitch or yaw, of the full-scale ship when slightly disturbed from steady forward motion.

PUBLICATIONS RECEIVED.

Physics in Industry. Lectures delivered before the Institute of Physics, by J. W. Mellor, A. E. Onley, and C. H. Desch, with a foreword by the Hon. Sir Charles A. Parsons. Vol. II, 48 pages, illustrations, plates, 8vo. Oxford University Press, 1924. Price, $1.

Companion to the First Edition of Chemical Synonyms and Trade Names, by William Gardner. 55 pages, 8vo. London, Crosby Lockwood and Son, 1925. Price, 7 shillings, 6 pence.

The Animal as a Converter of Matter and Energy. A study of the rôle of livestock in food production, by Henry Prentiss Armsby, and C. Robert Moulton. American Chemical Society Monograph Series. 236 pages, illustrations, 8vo. New York, The Chemical Catalog Company, 1925. Price, $3.

A Text-book of Inorganic Chemistry, edited by J. Newton Friend. Vol. 3, part 1, The Alkaline Earth Metals, by May Sybil Burr. xxvi–346 pages, figures, tables, 8vo. London, Charles Griffin and Company, 1925.

A Treatise on Physical Chemistry. A coöperative effort by a group of physical chemists. Edited by Hugh S. Taylor, D.Sc. Second printing—corrected. 2 vols., illustrations, 8vo. New York, D. Van Nostrand Company, 1925.

Syndicat de la Presse Technique Industrielle, Commerciale et Agricole de France. Programme, 1er Congrés International, September, 1925. 112 pages, portraits, quarto. Paris, Syndicat, 1925.

The Physical Chemistry of Igneous Rock Formation. A General Discussion held by the Faraday Society, the Geological Society, and the Mineralogical Society, October, 1924. 93 pages, illustrations, figures, tables, 8vo. London, Faraday Society. Price, 6 shillings, 6 pence.

The Crystalline State, by Sir William Henry Bragg. The Romanes Lecture delivered in the Examination Schools, 20 May, 1925. 31 pages, illustrations, 8vo. Oxford, Clarendon Press, 1925. Price, seventy cents.

Electricity and the Structure of Matter, by L. Southerns. 128 pages, illustrations, 12mo. London, Oxford University Press, 1925. Price, $1.

Introduction Géométrique a la Mécanique Rationelle, by Charles Cailler. xii–628 pages, figures, 8vo. Paris, Gauthier-Villars et Cie., 1924. Price, 60 francs.

United States Department of Commerce: Elimination of Waste: Simplified Practice Recommendation, No. 23, Plow Bolts. 10 pages, illustrations, tables, 8vo. No. 25, Hot-water Storage Tanks. 10 pages, illustrations, tables, 8vo. Washington, Government Printing Office, 1925.

United States Department of Commerce: Scientific Paper of the Bureau of Standards, No. 506, Theory and Interpretation of Experiments on the Transmission of Sound through Partition Walls, by Edgar Buckingham. 30 pages, figures, quarto. No. 507, A New Interference Apparatus for Testing Hæmacytometers, by C. G. Peters and B. L. Page. 20 pages, illustrations, plates, tables, quarto. Washington, Bureau of Standards, 1925.

Base Exchange in Soils. A General Discussion held by the Faraday Society, December, 1924. 93 pages, figures, tables, 8vo. London, Faraday Society, 1924.

Potentiometer Pyrometers. Catalogue No. 87, 1925, Leeds and Northrup Company. 55 pages, illustrations, quarto. Philadelphia, Leeds and Northrup Company, 1925.

Standard Specifications and Methods of Test for Materials, by C. L. Warwick. A paper presented at the First Pan-American Conference on Uniformity of Specifications. Lima, Peru, December 23, 1924. 19 pages, 8vo. Philadelphia American Society for Testing Materials, 1924.

Theorie und Konstantenbestimmung des hydrometrischen Flügels, von L. A. Ott. 50 pages, illustrations, tables, 8vo. Berlin, Julius Springer, 1925.

Water Supply System of Chicago, by John Ericson. 32 pages, illustrations, maps, 8vo. The Water Supply Problem in Relation to the Future Chicago, by John Ericson. 25 pages, diagrams, tables, 8vo. The Quality Problem in Relation to Chicago's Water Supply. Official Report to A. A. Sprague, by John Ericson. 29 pages, portrait, plates, table, 8vo. Chicago, Department of Public Works, 1925.

Ontario Department of Mines: Thirty-third Annual Report, part 3, 1924. 60 pages, illustrations, maps, 8vo. Toronto, 1925.

National Advisory Committee for Aeronautics: Technical Notes, No. 220, The Drift of an Aircraft Guided towards Its Destination by Directional Receiving of Radio Signals Transmitted from the Ground, by Edward P. Warner. 6 pages, figures, table, quarto. Washington, Committee, 1925.

ERRATA—BOOK REVIEWS.

"Introduction to Organic Research," by E. Emmet Reid, New York, D. Van Nostrand Company, 1924, reviewed in January, 1925, JOURNAL, page 129, originally announced at $5.00, price, $4.50.

"Foundations of the Universe," by M. Luckiesh, New York, D. Van Nostrand Company, 1925, reviewed in June, 1925, JOURNAL, page 852, price, $3.00, not $8.00 as given in the review.

"Recent Progress in Engineering Production," by C. M. Linley, June, 1925, JOURNAL, page 857, line 3, "requirements" should read "refinements"; line 5, "conservation" should read "conservatism."

Nickel Table Tops.—O. B. J. FRASER (*Ind. Eng. Chem.*, 1925, **17**, 604) recommends the use of pure sheet nickel for tops of tables in chemical laboratories. Very few reagents attack the nickel, and their action is extremely slow. J. S. H.

CURRENT TOPICS.

Saturation Pressures of Mercury up to 2000 Kg. per Square Cm. FRITZ BERNHARDT. (*Phys. Zeit.*, March 15, 1925.)—It was at the close of the nineteenth century that Cailletet, Colardeau and Rivière measured the vapor pressures of mercury and found their highest pressure of 162 atmospheres to correspond to a temperature of 880° C. This present investigation, carried out in Leipzig, extends the range to 1435° C., where the pressure amounts to 2020 kg./cm.^{2}.

Thermo-elements were used throughout to measure the temperatures. The investigation was made in three stages. In the first the mercury was contained in a capillary tube and was heated by an electric furnace. The meniscus of the liquid was observed from a distance of 4 m. through a telescope and the pressure and temperature corresponding to the commencement of boiling were determined. Since the glass tube was put under pressure in one direction only and was further utilized to the fullest extent, the experiment always ended in a violent explosion. A pressure of 70 kg./cm.^{2} at a temperature of 710° C. was the limit of this stage.

In the second stage the mercury was contained in a U-tube, one end of which was closed. This was placed in a bomb and subjected hydrostatically to pressure in all directions. The liquid was heated by an electric current flowing through wire wrapped about the closed end of the tube. Again observations of the mercury were made through a telescope directed toward quartz windows in the bomb. It is surprising to read, "Since temperatures of 800° were obtained, the liquid mercury was so cherry-red that it showed little contrast with the vapor-filled space illuminated by the lamp. The surface separating the liquid from the vapor was recognized by its seething." The range of this step in the process extended to a pressure of 170 kg./cm.^{2} at 925° C.

In the final stage direct observation of boiling had to be given up. The bomb was without windows and the mercury was contained in a U-tube open at both ends. The heating of the liquid was accomplished by sending an alternating current through it. The two legs of the U-tube were joined by a horizontal tube of smaller diameter carrying in its centre a cylinder that produced the result that there was merely a hollow cylinder of mercury connecting the two larger bodies of this element. It was this hollow cylinder that heated up most promptly and that first boiled. When this process did begin, some of the liquid was pushed aside by the resulting vapor. This was followed by an increase of the resistance of the circuit and by a lessening of the current strength and of heat development. Condensation of the vapor then took place and the process began

anew. From the variations in the current strength and voltage, as well as from temperature constancy and the sound of boiling, it was possible to tell when the mercury was actually in a state of ebullition. At a pressure of 2020 kg./cm.^{2} the liquid boiled at 1435° C. These values are therefore less than the critical pressure and temperature of mercury. G. F. S.

Thermo-electric Measurement of Cutting Tool Temperature. HENRY SHORE. (*Jour. Washington Acad. Sciences,* March 4, 1925.)—"Comparatively little has been attempted in this field, although calorimetric methods for observing the heat imparted to the chips have been used in lathe work and thermocouples inserted close to the cutting surface have been used in drilling tests." Lyman J. Briggs, of the Bureau of Standards, suggested the superior method employed in this investigation. This consisted in utilizing the contact of the tool with the work as the hot junction in a thermo-electric circuit, there being a sufficient difference between the two metals for them to serve as the two parts of such a circuit. Calibration tables are given for the e.m.f. developed for various differences of temperature between the material of the tool, Jessup high-speed steel, and the work metal, in one case cast brass and in another mild steel. As would be expected, the voltage developed for a given temperature was greater for tool-brass than for tool-mild steel. For the production of one millivolt with the former combination a difference of temperature of 96° C. was adequate, while for the second pair of metals no less than 315° was requisite.

In the actual experiment the work was insulated and the thermo-electric circuit was completed through it, the millivoltmeter and tool. In cutting cast brass with a speed of 16 ft. per min., a rise of 42° C. at the place of contact took place when .0024 lb. of chips per min. were removed. Upon the removal of twenty-four times as much in a minute the elevation of temperature became 106°. In cutting mild steel with a speed of 13.5 ft. per min. the temperature rise was 112° when the rate of removal was .001 lb. per min., and was 292° for a rate twenty-seven times as great. These results were obtained without the use of cutting fluids. In cutting both metals the rise of temperature with rate of removal is at first rapid, but later declines until at last but a slight addition to the temperature at the contact is caused by a material increase in the rate of removing metal from the work piece. G. F. S.

The Mass Spectra of Chemical Elements. F. W. ASTON. (*Phil. Mag.,* June, 1925.)—This contains an account of the application of the method of accelerated anode rays to the detection of isotopes. The author believes that the limit of usefulness for this type of apparatus has been reached and states that it has been dismantled to make room for a more powerful successor. Ten elements have been examined in addition to those previously analyzed. Indium

seems to be a simple element with no isotopes. Strontium along with the preponderating 88 contains an isotope of mass number 86, but only to the extent of 3 or 4 per cent. Barium persists in remaining something of a mystery with its non-integral atomic weight of 137.37 and with no constituent except 138. Lanthanum displayed no line except that of 139, and praseodymium none but that of 141. Both these elements therefore appear to be simple. Neodymium, on the other hand, contains isotopes of mass numbers 142, 144, 146 and possibly 145. Cerium has two isotopes, 140 and 142; zirconium, whose atomic weight has been rendered uncertain by the discovery of hafnium, has three isotopes, 90, 92, 94 and perhaps also 96. Cadmium had been attacked before but without success. It has no less than six isotopes, 110, 111, 112, 113, 114 and 116. Tellurium shows three, of mass numbers 126, 128 and 130. "Tellurium is unique among the elements so far analyzed as it seems probable that all its mass numbers form members of isobaric pairs. They are shared by xenon, the element of next higher even atomic number." Bismuth, having the heaviest atom so far weighed on the mass spectograph, turned out to be a simple element with mass number 209. A table is given in which the results of the analysis of the mass spectra of fifty-six elements are presented. Of the twenty-four that remain, some present very considerable difficulties. Radium, for example, is too expensive for this method. "The parent elements, thorium and uranium, appear most hopeless of all, for each is the heaviest member of a chemical group already found highly unfavorable to mass spectrum analysis. This is unfortunate for chemical evidence suggests that both are complex and a direct identification of their more important isotopes might be of the greatest value in explaining difficulties connected with the radio-active disintegration series." G. F. S.

The Effect of the Earth's Rotation on the Velocity of Light. A. A. MICHELSON and H. G. GALE. (*Astrophys. Jour.*, April, 1925.) —More than a score of years ago there was published in the *Philosophical Magazine* a plan of testing the effect of the rotation of the earth on the velocity of light. Two pencils were to interfere after they had traversed in opposite directions a path enclosing an area. An expression given in the article of 1904 for the difference of phase of the two pencils was found by L. Silberstein to need doubling. No attempt to try the experiment suggested was made until Doctor Silberstein convinced Professor Michelson of the importance of the matter. In order to reduce the expense the experiment was first tried in the open air on Mount Wilson in the summer of 1923. Interference fringes were obtained, it is true, but they were too unsteady to form the basis of reliable measurements. It was seen that for the successful conduct of the investigation a pipe line a mile long and a foot in diameter, exhausted of air, must be available. Funds for this were furnished by the University of Chicago

and officials of the City of Chicago rendered material assistance, so that at Clearing, Ill., twelve-inch water pipes were laid straight and level around a rectangle of land 2010 ft. from east to west and 1113 ft. from north to south. At one end there was a double line of pipes. At the four corners of the rectangle mirrors were set so that the light arriving along one edge of the rectangle was reflected away along the other edge. Light from an arc fell on such a corner mirror that was lightly coated with gold. Part of the light was reflected and another part passed through the coating. The two portions followed the contour of the rectangle in opposite directions and ultimately were brought to interference. A vacuum of from ½ to 1 inch of mercury was maintained in the pipe line with the result that the fringes were both distinct and steady. "The displacement of the fringes due to the earth's rotation was measured on many different days, with complete readjustments of the mirrors, with the reflected image sometimes on the right and sometimes on the left of the transmitted image, and by different observers." From 269 determinations the mean displacement came out .230 of a fringe, whereas the value calculated from the formula was .236. Thus the agreement is within the errors of observation. The theory that is thus confirmed is founded "on the assumption of a stationary ether as well as in accordance with relativity." G. F. S.

The Mineral Factor in Nutrition.—While the rôle of specific proteins and vitamins is the usual subject of a research in nutrition, O. E. REED and C. F. HUFFMAN (*Q. Bull. Agr. Exp. Sta.*, Michigan State College of Agriculture and Applied Science, 1925, 7, 125–132) have studied an equally important phase of the question, the mineral constituents of the ration. Their work was done on dairy cattle.

Reed and Huffman state: "Minerals are needed to develop the skeleton and for the proper functioning of the glands and organs of the animal body. When cattle were first domesticated, the minerals found in the natural feeding stuffs were ample to meet the needs for body maintenance and milk production, since only enough milk was produced to give the calf a start in life. The modern dairy cow, however, is a highly specialized machine for the production of milk, which is rich in mineral salts, especially lime and phosphorus. Not only has the tremendous increase in milk production caused a greater mineral requirement, but the feeds used to-day are often grown on soils depleted in the essential mineral elements, resulting in a deficiency in the crops grown on such soils."

Herbivorous animals like the horse and cow require proportionately more sodium chloride than do carnivorous animals. While but minute amounts of iodides are required, a deficiency of iodine results in a form of goitre, known as "big neck" in calves. If cows are fed timothy hay or are high milk producers, they must be supplied with additional lime (calcium). Bone meal is of value in the ration, since it supplies both calcium and phosphate, and also aids in the

utilization of other nutrients. The fat-soluble D vitamin, which aids in calcification and prevents rickets, is furnished by well-cured hay and by pasture. J. S. H.

The Degree-day, New Term to Measure a Hard Winter.—The "degree-day" is a new term invented by gas and fuel engineers for the purpose of measuring heating requirements, and appears for the first time in "House Heating," a book recently published under the auspices of the American Gas Association. With the increasing demand for city gas for house heating and industrial heating purposes, it became necessary to have some word or phrase which would enable engineers to make comparisons between heating loads under different climatic conditions or at different points. The "degree-day," which was the result, is the product of a degree of temperature and a time factor of one day.

In studying actual domestic heating conditions, the American Gas Association determined that the minimum temperature of bodily comfort in the home is reached when the mean daily temperature falls below 65° F. In other words, below this point, heat is required, as the average day-time temperature in the house drops below 70° F.

If, then, we have a mean daily temperature of 60° F., it is evident that, for this day, we can measure the heat requirements by the figure "5° days," whereas, had the mean temperature been 55° F., the requirement would be measured by "10° days," or would be twice as great. For a week or a month or a heating season, the aggregate heating load of any locality may be expressed in units that permit comparison with that of any other point. Likewise the heating requirements at any given place during the heating season may be compared with those of any other heating season by the "degree-day." So a "mild winter" or a "hard winter" becomes definable in accurate units that may be translated into B.t.u. Basing these units on the very accurate data of the Weather Bureau, a high degree of accuracy results.

Boundary Lubrication. SIR W. HARDY and IDA BIRCUMSHAW. (*Proc. Royal Soc.*, A 745.)—Amonton's law that the friction between two surfaces is proportional to the load is found not to hold in general when the surfaces are flooded with lubricant. When both plates are plane the coefficient of static friction grows less as the load increases until it attains a constant value, from which point onward the law of Amonton continues to be true. For example, with nonadecane as lubricant and a pressure of 96 g. per cm.^{2}, the coefficient for steady friction was .305. With a pressure of 2455 g./cm.^{2} it fell to .187 and with higher pressures it became constant at .184. While the coefficient is diminishing "the lubricant forms a layer between the faces which is several molecules in thickness, and is held in place by those ordinary capillary forces which cause fluids to run between two plates. The adjustment of the friction to the pressure is by

variation in the thickness of this layer; as it thins, friction rises, but not so fast as the pressure, and therefore the coefficient falls."

When the slider is placed on the plate flooded with lubricant some time elapses during which adjustments of the lubricant occur. In this "latent period" the coefficient grows larger for a minute or two.

When the slider is spherical the friction is proportional to the load and thus Amonton's law holds. G. F. S.

The Effect of Light on the Settling of Suspensions. C. G. T. MORISON, School of Rural Economy, Oxford. (*Proc. Royal Soc.*, A 746.)—In the course of soil investigations the material was stirred up with water and then allowed to settle. It was noticed that when the suspension was illuminated by electric lamps layers formed in the liquid. To avoid any temperature effect due to the lamps, a cell containing water was interposed between them and the suspension. The stratification then, far from disappearing, became more distinct than before. The color of the exciting light was next changed by means of filters and it became evident that the layers forming under the influence of red light were closer together than was the case with blue light. The possibility of this difference being due to a difference in the quantity of energy reaching the suspension in the two instances was examined experimentally and rejected. The author does not know the cause of the phenomena he has discovered.

G. F. S.

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